# EVALUATING EFFORTS TO REDUCE GUN VIOLENCE IN BALTIMORE:

# DRUG LAW ENFORCEMENT, CURE VIOLENCE, AND FOCUSED DETERRENCE

by
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## Abstract

Baltimore has been plagued by persistently high rates of firearm violence for decades, even as gun violence rates across the country have declined. City and law enforcement officials in Baltimore have employed various approaches to reduce homicides and nonfatal shootings. This research project evaluated the use of three strategies aimed at gun violence reduction in Baltimore: drug law enforcement, the Cure Violence model, and focused deterrence. Negative binomial regressions were used to estimate the effects of drug law enforcement and focused deterrence on homicides or nonfatal shooting counts at the police post level, while synthetic control analyses were used to estimate the effects of the Cure Violence model. Key informant interviews were also conducted to gain greater understanding of the focused deterrence program. Analyses indicate that drug law enforcement interventions were associated with no reductions in homicides but at times with increases in nonfatal shootings in the months following the interventions. Findings also reveal that the beneficial effects of the Cure Violence model have diminished since the program began in Baltimore. The focused deterrence replication was found to be harmful in one of the two police districts where implemented, and threats to the program's success, per key informant interviews, included concerns about accurate identification of individuals most responsible for violence, frequent leadership changes, a lack of financial support for law enforcement and of services for those sought for the intervention, and the lack of engagement by law enforcement with community members on issues of legitimacy, shared vision, and strategy. The dissertation project highlighted the importance of placing priority focus on violent individuals, adhering to program fidelity when implementing and executing evidence-based violence prevention programs, continuously monitoring, evaluating, and evolving

programs as needed, and addressing the contentious relationship in Baltimore between law enforcement and community residents.

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# Introduction

## Background

Gun violence in urban cities across America is one of the most challenging and devastating public health problems of our time. Although homicides and nonfatal shootings have dropped dramatically since the 1980s and 1990s (Cooper and Smith, 2011), gun violence continues to prematurely end and permanently alter the lives of tens of thousands of Americans each year. In 2016, 14,415 individuals were killed in firearm-related homicides, excluding encounters with law enforcement (Centers for Disease Control and Prevention, 2018). The percentage of all homicides that are committed by firearms has gradually increased over the past eighteen years, from 64% of homicides in 1999 to 74% of homicides in 2016. Data on total numbers of nonfatal shooting incidents in the United States are more difficult to obtain because federal crime reporting systems like the Federal Bureau of Investigations' Uniform Crime Reporting Program and the National Incident-Based Reporting System do not require jurisdictions to record nonfatal shootings separate from all aggravated assaults, and hospitals do not have a standardized system for tracking nonfatal shootings. However, the Centers for Disease Control and Prevention estimated nearly 89,000 non-law-enforcement-related shooting injuries in America in 2016, using data collected via the National Electronic Injury Surveillance System (Centers for Disease Control and Prevention, 2018). The burden of mortality and disability from homicides and nonfatal shootings falls disproportionately on black and brown communities in central cities across the country. In 2016, black Americans made up 14% of the population but accounted for 59% of all firearm homicides. The disparity is even higher for black males 15 to 34 years old, who, in 2016, made up 8% of the total (male and female combined) population for that age group but accounted for 60% of all homicides in that age category. Black Americans also accounted for over one-third of all nonfatal shooting injuries – nearly 33,000 - in 2016.

In addition to the trauma, pain, and suffering related to gun violence in communities, there are also great economic losses incurred by local jurisdictions when gun violence persists. In addition to millions of dollars in medical costs and productivity lost due to gun violence (Fowler et al., 2015), research has found that increases in gun homicides or shootings in neighborhoods negatively affect housing values (Tita, Petras and Greenbaum, 2006) and have adverse impacts on local business establishment numbers, employment, and sales (Irvin-Erickson et al., 2016). Thus, city leaders have multiple motivations for wanting to reduce gun violence and have sought various ways to increase public safety for their constituents.

Baltimore has long been plagued by high rates of homicides and nonfatal shootings; from 2003 to 2017, nearly 4,000 Baltimore residents lost their lives to homicide and over 8,000 were victims of nonfatal shootings (Baltimore Police Department, 2018). City and law enforcement officials in Baltimore, as in many urban cities, have attributed much of the gun violence to the illicit drug economy. For many years, the most visible and direct approaches employed by the Baltimore Police Department (BPD) to curb gun violence focused on the enforcement of drug laws to reduce violent crime associated with the drug trade. However, in recent years, the city has also implemented other programs – namely, street outreach/violence interruption and focused deterrence – that are designed to directly reach those individuals most at risk for violence perpetration and intervene in ways that ideally deter future violent behavior. While all three of these tactics have been used in cities across the United States that are similarly grappling to achieve steady reductions in gun violence, evaluations of the interventions have been shown to have varying degrees of success in different jurisdictions. Understanding the impact that these strategies have had on gun violence reduction in Baltimore can greatly inform policymakers and local leaders on how to best improve public safety for the city's residents.

## Literature Review

A review of the literature was undertaken to understand the theories, applications, and evaluations of three defined gun violence reduction approaches used in Baltimore over the past fifteen years: drug law enforcement, street outreach/violence interruption, and focused deterrence. The review began with searches of the PubMed, PsycINFO, and Google Scholar electronic databases. Initial search terms included "drug enforcement," "drug prohibition enforcement," "violence interruption," "Cure Violence," "Ceasefire," "focused deterrence," and "gun violence prevention." The review was limited to primarily research in the United States, although several international studies were considered when highly relevant to the topic of interest. Article titles and abstracts were reviewed to determine which were most appropriate. The reference lists in informative articles were used to identify key authors and additional works or review. Books, white papers, and grey literature were consulted.

The summaries of the literature reviewed for this dissertation project are organized based on the presentation of the three violence reduction strategies examined for this thesis work: drug law enforcement, violence interruption, and focused deterrence.

## Drug Law Enforcement

### The Relationship Between Drugs and Violence

The Controlled Substances Act of 1970 established U.S. federal drug policy for regulating and scheduling substances based on medical value, potential for misuse or dependence, and harmfulness, and laid the foundation for how violations of such regulations would be sanctioned (21 U.S. Code § 812, 1970). Most drugs known to be used and sold in the underground market, including marijuana, cocaine, heroin, opioids, and benzodiazepines, are thus known, particularly to law enforcement and legal practitioners, as "controlled dangerous substances" (CDS). The dangerousness of these substances, specifically as it relates to their potential for inciting or contributing to violence, has been studied for decades (e.g., Malmquist, 1971; Zahn and Bencivengo, 1974; Daniel et al., 1983). However, the pioneering research paper by Paul Goldstein (1985) on the drug-violence nexus provided a valuable typology for examining this association. Through in-depth interviews and ethnographic fieldwork notes, Goldstein determined that drugs and violence are related in ways that can be characterized by three different conceptual models: psychopharmacological, economically impulsive, and systemic. The psychopharmacological model suggests that through the short- or long-term intake of certain substances – examples include alcohol, barbiturates, and stimulants –, individuals may become irrational or excitable and exhibit violent behavior. The economically impulsive model posits that violence occurs when some drug users engage in economic-driven violent crime, such as robbery, in order to acquire money to support their drug use. The systemic model proposes that violence is intrinsic to markets for goods or services that are illegal and can erupt over disputes, robberies, or retaliation.

While much of the earlier research on the connection between drugs and violence focused on violence associated with the psychopharmacological or economic impulsivity models, more recent findings have been mixed (Resignato, 2000) and do not provide concrete evidence of a consistent relationship between overall drug *use* and violence (Goldstein, 1985; Goldstein et al., 1989). Recent research has shown that psychopharmacological effects of drugs on violence differ by substance, and most commonly used illicit drugs – marijuana, opioids, and cocaine – are not typically violence-inducing, while evidence to support a trend for economic impulsive motivations for gun violence is limited (McGinty, Chosky and Wintemute, 2016). However, numerous studies have found that a substantial number of violent incidents that can be linked to illicit drugs appear to be tied to the systemic dynamics of illegal drug markets (Goldstein et al., 1989; Klofas, Delaney and Smith, 2005; Levitt and Venkatesh, 2000; McGinty, Chosky and Wintemute, 2016). This suggests that the bulk of drug-related violence is not driven simply by drug consumption or addiction, and that more active players on the supply side of these markets interact with violence in different ways than do those on the demand side. The presumed link between the illegal drug trade and violent crime has motivated law enforcement agencies to devote time, resources, and strategic planning toward curbing illicit drug activity with the goal of reducing violence, particularly in communities that have high rates of both.

## Drug Prohibition Enforcement in the United States

The primary objective of the enforcement of drug control policy, both in the United States and throughout the world, is to reduce supply and demand of illicit drugs by disrupting supply and increasing the risk of arrest and incarceration for both sellers and buyers (Shepard and Blackley, 2005). In 2008, local and state law enforcement agencies spent approximately $26 billion on drug prohibition enforcement in America (Miron and Waldock, 2010), in addition to the estimated $3.5 billion spent by the federal government on domestic prohibition enforcement (Office of National Drug Control Policy, 2009). The results of increased enforcement of drug prohibition in America via drug-related arrests since the 1980s have been well documented. Between 1980 and 2014, the arrest rate for drug possession or use more than doubled, from

198.6 arrests per 100,000 in 1980 to 406.2 arrests per 100,000 in 2014, with a peak of 520.4 in 2006 (Snyder, Cooper and Mulako-Wangota, 2016). Although the arrest rate for drug sale or manufacture peaked in 1989 and gradually fell over time, it was still 68% higher in 2014 than at its lowest point in 1981 (Snyder, Cooper and Mulako-Wangota, 2016). In 2016, law enforcement agencies across the United States arrested nearly 1.6 million people for drug law violations, representing 14.7% of all arrests and the highest number by arrest category (United States Department of Justice, 2017).

Figure 1.1: Drug Possession Arrest Rates in the United States, 1980-2014

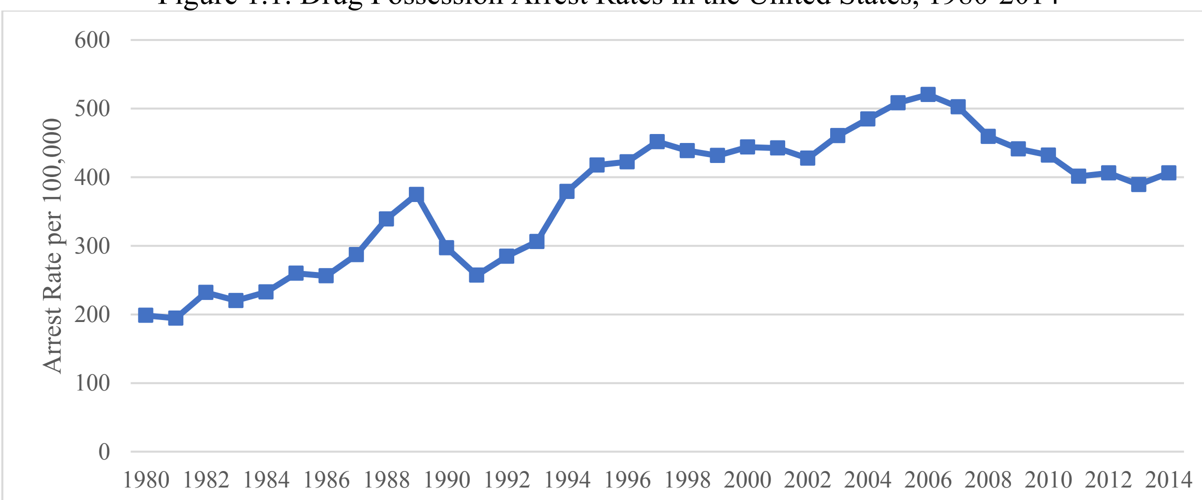



Figure 1.2: Drug Trafficking/Distribution/Manufacturing Arrests in the United States, 1980-2014

While many state and federal policymakers in the United States have expressed a desire in recent years to shift drug policy priorities in this country away from punitive actions towards drug users and low-level dealers (Office of National Drug Control Policy, 2015; Brownstein, 2016), law enforcement agencies continue to direct resources toward addressing the systemic violence of illegal drug markets. In 2013, for example, over 13% of all police departments in the country and more than nine out of ten police departments that serve populations of 100,000 or more – including 100% of police departments in cities with over 1,000,000 people - were participating in multiagency drug task forces (Reaves, 2015). These task forces focus their operations on local-level drug possession and distribution violations, in addition to more sophisticated drug rings that supply narcotics across jurisdictions and state lines. Furthermore, since 2017, the Trump administration has directed federal prosecutors to seek the maximum penalties available in cases against drug dealers (Ford, 2017) and to even use death penalty statutes when prosecuting certain drug-related cases, such as those involving specific racketeering activities or "extremely large" amounts of drugs (Office of the Attorney General, 2018).

In addition to reducing illegal drug supply and demand by increasing the risk of arrest and incarceration for both sellers and buyers, another intention of drug prohibition enforcement is to restrict supply sufficiently to reduce availability and increase price to the point where drug use becomes less attractive (Caulkins et al., 2005). However, this relationship between supply, demand, and retail prices for illicit drugs is far from straightforward, complicating law enforcement's direct ability to influence price or demand. Researchers have detailed numerous ways in which drug law enforcement might lead to either increased drug prices and reduced consumption (Bright and Ritter, 2011; Caulkins et al., 2005) or reduced drug price and competitive markets (Poret, 2012). Other research has found no evidence that increasing the risk of seizure, arrest, or incarceration raises the price of illicit drugs (Pollack and Reuter, 2014; Bushway and Reuter, 2011). Thus, the notion that drug law enforcement at the local level successfully disrupts illicit drug markets by increasing real or perceived risks and costs to sellers and/or buyers has not been consistently supported in the research to-date.

Illicit Drug Market-Related Violence and Drug Law Enforcement

Illegal or underground markets, including those involving the illicit drug trade, are not necessarily inherently violent. For example, illicit marijuana markets in general have not generated remarkable levels of violence over disputes or competition (Reuter, 2009). Additionally, the United States has one of the highest rates of illegal drug use in the world, yet there is not a consistently correlated high rate of violence wherever illegal drug use exists. However, numerous studies have found that the underground drug market is a key source of violence, particularly in urban areas, such as New York City (Goldstein et al., 1997; Johnson, Golub, and Dunlap, 2000), Chicago (Levitt and Venkatesh, 2000), Washington, DC, and Los Angeles, (Ousey and Lee, 2004). Various methods or situations by which this systemic violence may occur have been supported through research. Being involved in an illegal activity with punitive consequences for involvement leads people to be less likely to have disputes resolved or retributions and reparations paid by traditional or governmental institutions. (Jacques and Allen, 2014). The formal, traditional systems for dispute and conflict resolution increase the risk of being arrested or punished oneself for his or her involvement in the illegal trade, or the resolution that comes from formal justice systems is not deemed satisfactory, due to the inability of the system to fully redress the victim in money or drugs as compensation for illegal market participation. Engagement with law enforcement or the criminal justice system also increases one's risk of being labeled an informant. With the additional knowledge that police and prosecutors are typically less aggressive in their pursuit of justice for individuals that engage in illegal activity themselves, individuals in the illegal drug market see the involvement of governmental entities like law enforcement as too risky and having too high of an opportunity cost (Jacques and Allen, 2014; Jacques and Wright, 2013). This constellation of factors can lead to two distinct types of systemic drug market violence: predation, which is the taking of one's possessions or market share turf through robbery, burglary, or assault with the assumption that formal justice will not be sought by the victim; or retaliation, which is the pursuit of informal justice and could include vigilantism. Several studies have found evidence of both predatory and retaliatory drug market-related violence, or the use of violence to gain or maintain market share, as well as to settle drug-related disputes (Donohue and Levitt, 1998; Brownstein, Crimmins, and Spunt, 2000; Blumstein, 1995).

The association between the illicit drug market and violent crime has helped to justify the substantial increase in the late 1980s and 1990s in resource allocation and commitment to drug control policies by local, state, and federal law enforcement agencies nationwide (Sacco, 2014). Billions of dollars are spent in the United States each year at the local, state, and federal levels on domestic enforcement of drug control policies (Office of National Drug Control Policy, 2015), yet there are relatively few published studies that have quantitatively examined the impact that drug law enforcement in the United States has had on decreasing supply, consumption, or violence associated with illicit drug markets.

A 2006 meta-analytical review of street drug law enforcement examined the reduction of street-level drug problems by four types of drug law enforcement approaches: community-wide policing (general, nonspecific partnerships with non-law enforcement entities), problem-oriented/partnership policing (directed partnerships with non-law enforcement entities in geographic hot spots for drug activity), hotspots policing (law enforcement focus on geographic hot spots for drug activity), and standard, nonspecific law enforcement efforts (e.g., routine patrols, arrests) (Mazerolle, Soole and Rambouts, 2006). The analysis included studies that evaluated interventions launched, managed, and/or implemented by local law enforcement to prevent or reduce illegal drug use, illegal drug selling, and associated problems in drug selling locations. The researchers assessed pre- to post-test intervention effects on a number of outcome variables, including drug- and nondrug-related calls for service, property crimes, and violent offenses. None of the reviewed studies that measured impact on reported crimes against persons or calls for service for violent crimes demonstrated significant impact.

A 2011 systematic literature review to assess the relationship between drug law enforcement approaches and drug market-related violence reduction yielded eleven United States-based studies that employed longitudinal data analyses of empirical data (Werb et al., 2011). Nine of the eleven quantitative evaluations they reviewed reported a significantly positive (harmful) relationship between drug law enforcement and drug market violence. For instance, an in-depth economic analysis on financial activities of a drug-dealing street gang in Chicago found that the lack of official dispute settlement channels and pressure from drug law enforcement contributed to high levels of violence (Levitt and Venkatesh, 2000). A longitudinal observational study of 67 counties in Florida and found that drug enforcement measures such as resource allocation, number of sworn officers, and number of drug arrests and convictions were positively correlated with Part I violent crimes (Benson, Rasmussen and Kim, 1998). An examination of violent crime rates to the proportion of drug arrests to total arrests in 24 United States Metropolitan

Statistical Areas (MSAs) in 1992 and 1993 revealed a significant and positive association between the two rates (Resignato, 2000).

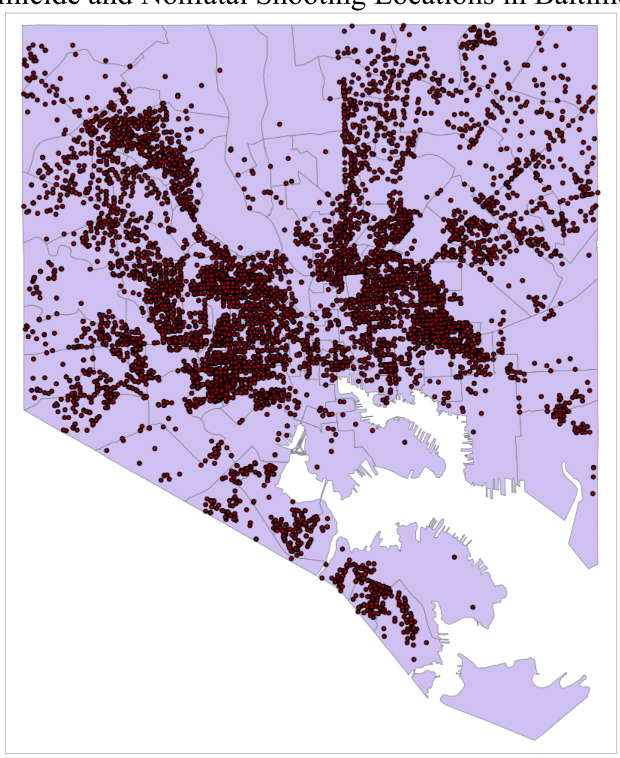
Looking beyond the United States, there is also evidence that drug prohibition enforcement may contribute to violent crime. The Johns Hopkins Lancet-Commission on Drug Policy and Health reviewed existing literature on public health issues stemming from drug policy and found that much of the drug-related violence worldwide is associated with the protection of illicit drug markets by drug cartels against armed government or paramilitary forces. The researchers also highlighted that "some experts have suggested that heavy crackdowns by drug police can lead to major increases in violence when disruption of a criminal network leads rival groups to intensify their efforts to capture the territory of the weakened group" (Csete et al., 2016, p. 7), using Mexico and Central America as examples. Mexico has experienced a historic rise in homicides in since 2006, unprecedented for any country not formally involved in war. This dramatic increase in homicide has been linked to the government's decision to utilize the military to fight drug traffickers in civilian areas (Heinle, Ferreira and Shirk, 2015). Other research found that approximately 25% of Colombia's homicide rate between 1994 and 2008 could be explained by law and militarized enforcement of drug laws surrounding the illicit cocaine trade, and that homicides spiked when drug law enforcement was most intense (Mejia and Restrepo, 2014). While it is important to recognize that there are substantial differences between the United States and Mexico or Colombia, given the findings in existing literature, the potential correlations between drug law enforcement tactics and violence associated with the illicit drug trade in this country cannot be ignored.

**Violence Interruption**

Violence as a Contagion

The Cure Violence model for violence reduction is based on the fact that, like other infectious diseases, violence is contagious (Cure Violence, 2018). Similar to an infectious disease, violent behavior exhibits characteristics indicative of clustering, transmission, and spread effects (Institute of Medicine, 2013). In virtually any city across America and the globe, violence is not distributed uniformly across geographic areas. The map in Figure 1.3 illustrates the locations of homicides and nonfatal shootings in Baltimore City between 2003 and 2017, illuminating the clustering of these events.

Figure 1.3: Homicide and Nonfatal Shooting Locations in Baltimore, 2003-2017



As with many other health disparities, a concentration of homicides is generally found in highly disenfranchised and structurally disadvantaged communities experiencing other poor quality of life indicators, including high rates of residential segregation and poverty, high rates of unemployment or underemployment, and extremely limited economic opportunities. These communities are also often plagued by a historically consistent prevalence of violence, supporting the notion that violence does not occur randomly and unpredictably. Substantial research has shown that exposure to violence increases one's likelihood of perpetrating or even being victimized by violence in the future, validating the idea that violence can be transmitted from person to person (Institute of Medicine, 2013; Bond and Bushman, 2017). Furthermore, the persistence of violence in these communities suggests that violence is spread through underlying social and behavioral norms in these areas. Research has shown there is a strong relationship between deviant behavior and an individual's peer group, or social network (McGloin and O'Neill Shermer, 2009), and that a considerable amount of violence, particularly in urban areas, is concentrated among a small number of high-risk individuals (Papachristos, Braga and Hureau, 2011; Braga and Wiseburd, 2015; Papachristos, Wildeman and Roberto, 2015). If the spread of violence is facilitated via social and behavioral norms, then reducing violence, or disrupting the transmission, requires challenging norms and expectations that support resolving conflict or anger with violence (Cure Violence, 2018). Using these basic understandings of the dynamics of violence and likening violence to a contagious disease that acts and thus can be treated like any other infectious disease, Cure Violence aims to identify and interrupt the transmission of violence by changing behaviors and norms related to the acceptance of violence as a means of handling conflict.

Cure Violence Program Model

The Cure Violence program is a multi-component, community-level intervention that employs street outreach workers to develop relationships and engage with individuals at high risk for committing or being victims of violence. The program model entails three key components: interrupting transmission of violence by mediating conflicts and limiting the likelihood of retaliation; identifying those at highest risk of perpetration of violence and reducing their risk through behavior change and linkage to needed services; and changing community norms around violence through community organization and anti-violence messaging (Cure Violence, 2018). The outreach workers often themselves have had extensive criminal histories, previous incarcerations, and/or former gang affiliations and are generally well known in the communities in which they work. They have also undergone their own personal transformations and are tasked with helping to steer the individuals with whom they work away from lives of violence. Being formerly engaged in and familiar with the very behaviors and activities they hope to change increases the likelihood that the outreach workers will be seen by their intended clients as credible messengers and thus potentially trustworthy resources. The outreach staff, then, serve as role models who can exhibit prosocial behavior while also helping to link individuals to critical supports and services (educational, financial, health, job training, etc.). Through the relationships built by the outreach workers and the connections to systems and supports that can help address the needs of clients and their families, the at-risk individuals will ideally choose positive paths of development and conflict resolution. The program also employs special outreach workers who operate primarily as violence interrupters, working to identify, resolve and de-escalate potentially dangerous conflicts that could lead to shootings. These staff play an essential part in working with individuals with high risk of violence perpetration to teach them how to resolve conflict and situations that elicit negative affect without resorting to lethal violence, recognizing that while the program cannot always intervene at the initial act of violence, it can minimize the spread of violence by interrupting transmission through conflict mediation. The third key component of the intervention involves community mobilization and addressing social norms that perpetuate violence. The program staff help to organize responses to homicide and nonfatal shooting incidents and engage with community partners to promote anti-violence messages and an intolerance for using guns to resolve conflicts, often through public events or social campaigns (Butts et al., 2015).

The Cure Violence program model has been implemented and evaluated in numerous cities around the United States. Impact studies of the program have found mixed results of the intervention's success at reducing gun homicides and nonfatal shootings (Butts et al., 2015; Cerdá, Tracy and Keyes, 2017). For example, in Chicago, an interrupted time series analysis of the program found that the program was associated with 16-28% reductions in nonfatal shootings in four of seven Cure Violence communities and variation across sites in the program's impact on outcomes such as gang involvement in homicide and retaliatory shootings by gang members (Skogan et al., 2008). An early analysis of a program site in New York City found that the program was associated with an 18% reduction in nonfatal shootings, although the decline itself was only significant when compared to the change in gun violence rates in comparison sites (Picard-Fritsche and Cerniglia, 2013). A more recent evaluation of two program sites in New York City found statistically significant gun injury decreases (32-65%) in the neighborhoods where the programs had been implemented (Delgado, Alsabahi and Butts, 2017). An independent evaluation of Philadelphia's Cure Violence program found that the intervention led to a statistically significant 30% reduction in nonfatal shootings after two years (Roman et al., 2018), while an analysis of a program based upon the Cure Violence model in Phoenix, Arizona, found that the intervention was actually associated with a significant increase in nonfatal shootings and a significant decrease in assaults (Fox et al., 2014).

Several evaluations of Cure Violence model replications have also examined the program's influence on attitude changes about the acceptance of violence to handle conflicts and the program's ability to engage the community to reduce gun violence. In New York City, researchers surveyed young men in eight neighborhoods with matching crime rates and demographics; four of the neighborhoods had Cure Violence programs. The researchers recruited and surveyed respondents on whether they would use violence in various conflict scenarios and found significant differences in the willingness to use violence among young men in the neighborhoods with Cure Violence programs (Delgado, Alsabahi and Butts, 2017). Another study in New York City used a convenience sampling recruitment strategy to survey residents in public places, such as parks and street corners, immediately after the implementation of a Cure Violence program and again one year later. The survey respondents reported an increased awareness of antiviolence messages in the community and increased confidence in the intervention but also no change in their feelings of safety one year following the program's implementation (Picard-Fritsche and Cerniglia, 2013).

Cure Violence ("Safe Streets") in Baltimore

In 2007, the Baltimore City Health Department received a $1.6 million grant from the U.S. Department of Justice to implement the Cure Violence program model in Baltimore (Webster et al., 2013). The program, named Safe Streets, serves youth ages 14 to 25 who are at the highest risk of perpetrating violence and living in communities that experience high rates of gun homicides and nonfatal shootings. Safe Streets was initiated in single police posts within four neighborhoods – McElderry Park, Elwood Park, Madison-Eastend, and Cherry Hill - between 2007 and 2008. A fifth site was planned in a police post in the Union Square neighborhood but encountered substantial implementation challenges and was terminated within one year. The Safe Streets program expanded to Baltimore's Mondawmin community in 2012 and to Lower Park Heights in 2013. Following the civil unrest that occurred following the death of Freddie Gray, Jr., in April 2015, and the sharp increase in gun violence across the city but particularly in West Baltimore, city officials opened a Safe Streets site in the Sandtown-Winchester community in 2016.

Researchers published an analysis of Safe Streets' impact on gun violence in the first four communities, comparing homicide and nonfatal shooting incident rates in intervention sites to rates in bordering areas and other areas with high rates of violence, and controlling for law enforcement activities and arrests (Webster et al., 2013). The researchers found that only the Cherry Hill program was associated with significant reductions in both homicides and nonfatal shootings. Cherry Hill saw a 56% reduction in homicides and a 34% reduction in nonfatal shootings, and neighboring communities also experienced significant homicide reductions. However, the other program sites had mixed success. McElderry Park saw a 26% decrease in homicides but a 22% increase in nonfatal shootings, while Elwood Park saw no significant change in homicides but a 34% reduction in nonfatal shootings. The Madison-Eastend community experienced a large increase in gang violence during Safe Streets' operational period and saw significant increases in homicides but significant decreases in nonfatal shootings (Webster et al, 2013).

Following the first evaluation, researchers have also examined the Safe Streets program's influence on youth's attitudes about the acceptability of the use of guns to settle conflicts (Milam et al., 2016a). The researchers surveyed youth ages 18-24 on randomized street blocks in Lower Park Heights and a comparison, non-intervention community. Respondents were surveyed in two waves, once pre-implementation of Safe Streets in Lower Park Heights and again one year post-implementation, using a 37-question validated instrument to measure changes in personal attitudes and shifts in social norms related to violence and retaliation (Milam et al., 2016b). The surveys were anonymous and self-administered. Using explanatory structural equation modeling and chi-squared tests to assess differences in the first and second survey waves, the researchers found that respondents in Lower Park Heights had significant improvements in attitudes towards violence and a greater magnitude of improvement in violent attitudes to personal conflict than did those in the intervention community. (Milam et al., 2016a). They also found that the presence of anti-violence signs and interactions with Safe Streets workers led to significant increases in nonviolent attitudes toward conflict.

**Focused Deterrence**

<u>The Concentration of Violence</u>

As earlier mentioned, research has shown a strong relationship between deviant behavior and an individual's social network (McGlowin and O'Neill Shermer, 2009). There has also been consistent evidence demonstrating that a considerable amount of violence, particularly in urban areas, is concentrated among a small number of high-risk individuals (Papachristos, Braga and Hureau, 2011; Braga and Wiseburd, 2015; Papachristos, Wildeman and Roberto, 2015). This research has largely been done through the creation of social networks based on police records of co-arrests; in other words, if two or more individuals were arrested together (co-offend), the assumptions are that they have some affiliation with one another, and that their involvement in illegal behavior suggests that they are part of a larger social network of individuals that may also engage in risky behavior (Papachristos, Wildeman and Roberto, 2015). This approach is limiting in that it depends solely on police observation to detect social network participation. In doing so, it substantially underestimates the extent of one's network and provides a conservative approximation of the relationship between violent victimization and one's social network. Nonetheless, the findings regarding this association show just how highly concentrated these networks are. For example, in a study of shootings in Newark, New Jersey, nearly one-third of the city's shootings occurred among just four percent of the population, and researchers found that being directly connected to a gang member increased non-gang associate's probability of being shot by 94%, while being a gang member in the network increased odds of victimization by 344% (Papachristos et al., 2015). A similar analysis in Chicago revealed that 70 percent of gunshot victims during the study time period were in a social network involving under six percent of the city's entire population and that being in the network component with a homicide victim increased homicide risk by 900%, with each unit increase in social distance removed from a victim, or each additional "handshake" or measure of social distance away from a homicide victim, decreasing odds of victimization by 57 percent (Papachristos, Wildeman and Roberto, 2015).

Focused Deterrence

Focused deterrence, or "pulling levers" policing, is a crime reduction strategy designed to identify and reach individuals and groups believed to be most responsible for crime in a given area. Similar to the Cure Violence model, the intervention is supported by research showing that a substantial amount of crime in a given community or city is committed by a small percentage of individuals. However, focused deterrence relies on the threat of law enforcement intervention for those who do not heed warnings of severe consequences if crime continues. Focused deterrence has been used to address various crime concerns, although it has most frequently been used to identify and connect with violent, group-involved individuals in communities with high rates of gun violence (Braga, Weisburd and Turchan, 2018).

The program model is comprised of several key components:

1. It is directed toward a specific crime concern, such as gang shootings or illicit drug dealing.
2. It involves the formation of a cross-agency enforcement team including local police, city, state and federal prosecutors, federal law enforcement agencies, and parole and probation departments.
3. It relies on intel and knowledge from front-line police officers and detectives to identify key group-involved individuals responsible for the concerning crimes.
4. The enforcement team then develops a strategy to direct at those individuals and groups and influence their behavior by using all possible legal actions against them.
5. Once the strategy has been determined, the intended population is directly contacted, usually by way of a group “call-in” or personal notification meeting, and informed of the increased scrutiny to which they are being subjected and why. The individuals and groups are also told what severe sanctions and enforcement actions will be taken against them if the illegal behavior does not stop, as well as what actions they can take to avoid the harsh penalties.

6. The message from law enforcement that the unacceptable behavior will no longer be tolerated is accompanied by messages from community members who have been negatively impacted by the behavior and implore the individuals to cease the harmful activity.
7. A focused deterrence program also includes an offer of services from local agencies and community-based organizations to support lifestyle and behavior changes, including substance use disorder treatment and assistance finding employment (National Network for Safe Communities, 2018; Braga, Weisburd and Turchan, 2018).

The focused deterrence program model was first developed and implemented in Boston under the leadership of criminologist David Kennedy in the 1990s; it has since been replicated in dozens of cities across the country. A 2018 systematic review found that 19 of 24 evaluations of focused deterrence programs were associated with strong, statistically significant crime reductions where implemented (Braga, Weisburd and Turchan, 2018). For example, Project Safe Neighborhoods, in Lowell, Massachusetts, was associated with a 44% reduction in gun assault incidents and no displacement effects of the intervention (Braga et al., 2008). The Group Violence Reduction Strategy in New Orleans, Louisiana, was credited with a 17% reduction in total homicides and firearm homicides, a 17% reduction in nonfatal firearm assaults, and a 32% reduction in group member-involved homicides (Corsaro and Engel, 2015). Boston has implemented two versions of the group-member violence reduction strategy - once to address youth violence and again to curb a growing gang violence issue - and saw significant reductions in gun violence in both iterations of the program (Braga et al., 2001; Braga, Hureau and Papachristos, 2014). The authors of the systematic review noted that the strongest crime reduction impacts have come from focused deterrence strategies concentrated on the most violent actors, and that focused deterrence approaches aimed at reducing crime associated with illicit drug markets had the smallest effects (Braga, Weisburd and Turchan, 2018). Additionally, several evaluation studies have found that threats to treatment fidelity can occur at several stages of program implementation, potentially undermining the success of the intervention in a given community (Corsaro and Brunson, 2013; Fox, Novak and Yaghoub, 2015; Saunders, Kilmer and Ober, 2016).

**Research Rationale**

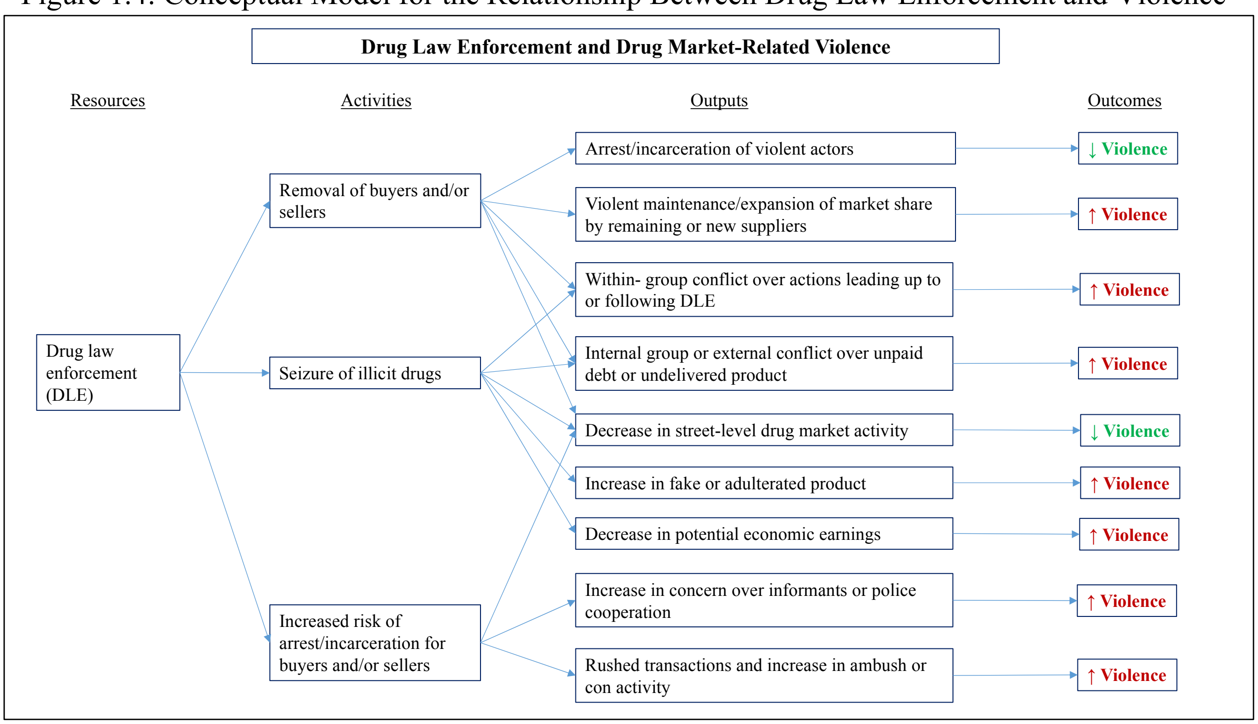
In addition to the fact that relatively few published research studies on the relationship over between law enforcement-led drug market disruptions and violent crime in the United States exist, the quantitative research is primarily based on data from the 1980s, 1990s and very early 2000s. Furthermore, the majority have either been descriptive studies in one city (Goldstein et al., 1997) or cross-sectional analyses across multiple localities (Rasmussen, Benson and Sollars, 1993; Benson, Leburn and Rasmussen, 2001), so they do not allow for estimations of changes in trends. Over the past 20-30 years, substantial changes in the prices, purities, and substances of choice in illegal drug markets in the United States (Fries et al., 2008) may have led to different responses to drug law enforcement than what was previously seen during the powder and crack cocaine epidemics and related high rates of homicides and shootings of the 1980s and 1990s (Reuter, 2009). The latest systematic review article on drug law enforcement noted that an analysis of the association between homicide rates and drug law enforcement expenditures in the early 2000s suggested a possible reversal in the positive association and posited that the drug control policies and actions in the past twenty years may have contributed to reductions, rather than increases, in homicides (Werb et al., 2011). Given the incredible resources devoted to drug law enforcement each year, its effectiveness on reducing gun violence is an important one for policymakers and law enforcement agencies of all levels to understand.

The Johns Hopkins Center for Gun Policy and Research recently examined various interventions, including drug law enforcement, violence interruption, and focused deterrence, in Baltimore that were implemented to reduce gun violence (Webster, Buggs and Crifasi, 2018). The researchers' findings from their analysis on drug law enforcement were largely in line with prior research conclusions across the United States. Major drug busts, as well as drug possession and drug trafficking arrests, were not found to significantly reduce homicides or nonfatal shootings, while increased numbers of drug trafficking arrests, or "surges" in arrests, were associated with significant increases in nonfatal shootings. The researchers found no aggregate effects of all Safe Streets sites on homicides or nonfatal shootings from 2007-2017 and only statistically significant reductions in homicides in one neighborhood. They also found that focused deterrence, known as Operation Ceasefire Baltimore, was not associated with decreases in either outcome. However, the researchers noted that future research would examine some of the interventions with analytic methods designed to more finely isolate intervention effects and determine if the findings in their analysis were related to actual program impact or statistical noise. This dissertation study further explored the findings of the recent Johns Hopkins Center for Gun Policy and Research report by incorporating additional data and different statistical approaches to provide a greater understanding of the effectiveness of drug law enforcement, street outreach/violence interruption, and focused deterrence on reducing gun violence in Baltimore.

## Conceptual Models

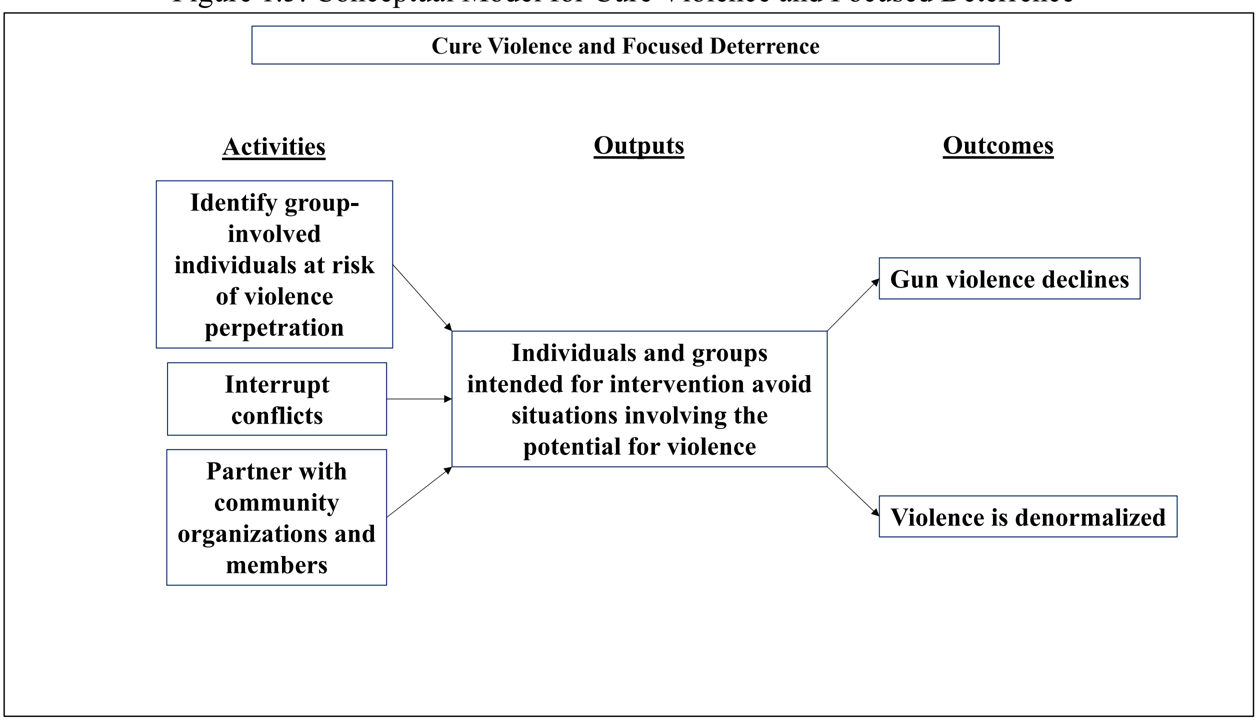
There is not an existing conceptual model that illustrates all the possible relationships between drug law enforcement and drug market-related violence. Thus, one has been created to show the existing theoretical and evidenced connections based on sociological and criminological research. The model outlines how the primary activities of drug law enforcement – namely, apprehending drug sellers/buyers, increasing risk of apprehension for drug sellers/buyers, and seizing illicit drugs – might precipitate a host of secondary effects, or outputs, which have the potential to either incite or suppress violence. It is possible for more than one activity or output to simultaneously occur. There is also the possibility that one output could precede another in time. For example, the removal of buyers and/or sellers via a drug bust could initially reduce violent activity, but conflicts over unpaid debts or competition for customers could result in an increase in violence over time.

Figure 1.4: Conceptual Model for the Relationship Between Drug Law Enforcement and Violence



A conceptual model that supports the Cure Violence and focused deterrence interventions is shown in Figure 1.5. Though their mechanisms for changing behavior and achieving program outcomes are different, Cure Violence and focused deterrence program models are both directed at the individuals most at risk for violence perpetration. They also both aim to disrupt violence via a reliance on community partnerships to provide assistance to those individuals in need. The model in Figure 1.5 was slightly adapted from one created by Dr. Caterina Roman at University of Pennsylvania, who was integral to an evaluation of both strategies implemented in Philadelphia.

Figure 1.5: Conceptual Model for Cure Violence and Focused Deterrence



## Research Questions and Objectives

The research questions and objectives for this study were as follows:

Question #1: Has the enforcement of drug law prohibitions in Baltimore been associated with a decrease in gun violence in Baltimore?

Objective 1.1: Examine the spatial-temporal association of drug law enforcement interventions – drug possession arrests, drug trafficking/distribution arrests, and major drug busts – and gun violence in Baltimore.

Objective 1.2: Examine whether there are differential effects of the spatial-temporal association between major drug busts and gun violence in Baltimore if the arrests in those busts are of individuals alleged to be violent actors in the illicit drug trade.

Objective 1.3: Examine whether the spatial-temporal association between major drug busts and gun violence in Baltimore differs based on the involvement of at least one federal agency in the arrests.

Objective 1.4: Determine if the spatial-temporal association between drug law enforcement in Baltimore and gun violence changed following the civil unrest in April 2015 following the in-custody death of Freddie Gray, Jr, given that during the unrest, law enforcement reported that hundreds of thousands of prescription pills were stolen from dozens of pharmacies throughout Baltimore and flooded into the streets, causing major drug market disruptions and likely driving much of the subsequent violence, and that following the unrest and the appointment of a new police commissioner, the Baltimore Police Department acknowledged a shift in strategy for drug law enforcement, publicly stating that the department was prioritizing the arrests of violent actors in the drug market.

Question #2: Has the Safe Streets program continued to reduce gun violence in Baltimore?

Objective 2.1: Evaluate the impact that the Safe Streets program has had on gun violence in each of the sites where the program has been implemented.

Objective 2.2: Examine whether Safe Streets site-specific program effects on gun violence changed following the civil unrest in Baltimore in April 2015.

Objective 2.3: Examine whether the program effects of Safe Streets have changed over time in the communities where the program has been operating for years.

Question #3: What was the impact of Operation Ceasefire Baltimore on gun violence, and how might insights from individuals involved with the program enhance our understanding of the intervention's effects?

Objective 3.1: Examine the spatial-temporal association between Operation Ceasefire Baltimore and subsequent gun violence in the districts where the call-ins occurred.

Objective 3.2: Supplement the quantitative findings from the Operation Ceasefire Baltimore analysis with key informant interviews of personnel instrumental to the program.

## Methods

### Data sources

To analyze the impact of the interventions in this research on gun violence in Baltimore, it was decided that homicides and nonfatal shooting counts would be the outcomes of interest; they are the most reliably tracked incidents of violence, given their gravity and more acute nature. Furthermore, over 84% of all homicides in Baltimore in the past six years were committed with firearms (Baltimore Police Department, 2018). The primary data used for the dependent variables in all three studies within this dissertation were homicide and nonfatal shooting data from the Baltimore Police Department. The Baltimore Police Department (BPD) directly provided individual-level information on all homicides and nonfatal shootings between January 1, 2003, and November 30, 2015. The homicide and nonfatal shooting data from BPD included the date and physical location of the incident, as well as the victim's date of birth, gender, and age. Data for homicides and nonfatal shootings occurring between December 1, 2015, and December 31, 2017, were obtained through the city of Baltimore's Open Data catalog for BPD Part I Victim Based Crime Data (Open Baltimore, 2018). These data are updated weekly by BPD and, in addition to the incident date, include the street block, weapon type, premise, and XY coordinates of each reported crime incident, but they do not include any victim-specific information.

To measure BPD's enforcement of drug law prohibitions, individual-level arrests for drug possession and drug trafficking or distribution violations between January 1, 2003, and December 31, 2015, were obtained directly from BPD. To attempt to account for any influence that BPD's utilization of proactive policing and prioritization of illegal weapon carrying may have had on homicide or nonfatal shooting counts, we also obtained data on weapon possession arrests for the same time period from BPD. The drug- and weapon-related arrest data only included the arrest date, time, location, and arrest charge(s). Drug possession, drug trafficking/distribution, and weapon possession arrests from January 1, 2016, through December 31, 2017, were obtained by downloading all arrest data from Baltimore's Open Data catalog for BPD Arrests (Open Baltimore, 2018) and then using relevant key words to extract arrests for drug possession, drug trafficking/distribution and weapon possession. The Open Baltimore database is updated with individual-level arrest data weekly by BPD and includes the arrestee's gender and race, in addition to the arrest date, time, street block location, XY coordinates of the arrest, and arrest charge. The Open Baltimore database for BPD arrests includes a statement that the data on the site represent the most serious arrest charge of an individual processed at the city's Central Booking and Intake Facility, and that arrests for individuals who were processed at the city's Juvenile Booking Facility are excluded from the site. This limitation suggests that some arrest data that could be relevant to our analysis may not be included in the Open Baltimore database. However, a comparison by count and arrest type (drug possession, drug trafficking, or weapon possession) of Open Baltimore arrest data from January 1, 2012, through December 31, 2015, with the corresponding arrest data provided directly by BPD yielded a net 95% match between the two datasets, so Open Baltimore data was deemed a suitable substitute for this project.

To measurer larger-scale, more resource-intensive and coordinated drug law enforcement activity, information was extracted from articles from *The Baltimore Sun*, the city's largest print media source, that detailed arrests and indictments for illegal drug sales between January 1, 2003, and December 31, 2017. A ProQuest Central search was conducted using the Baltimore Sun publication identification number (46036) and the following keywords: ("drug" OR "narcotic" OR "cocaine" OR "heroin" OR "marijuana") AND ("arrest" OR "prosecution" OR "prosecute" OR "indict" OR "indictment"). The ProQuest Central search yielded 3,693 articles, of which 809 were reviewed. Any event documented in a *Baltimore Sun* article that mentioned an arrest and/or indictment of individuals for drug law violations as a "bust." The ProQuest Central article search was supported by an article search in Google, using the same keywords as above but adding "Baltimore" and each year of the study period in separate searches, to ensure that no major drug-related arrests were missed. A drug bust was categorized as "major" if any of the following conditions were met: 1) five or more individuals were arrested in the bust; 2) charges included drug conspiracy, drug kingpin statute, running a violent drug gang, continuing a criminal enterprise, or Racketeer Influenced and Corrupt Organizations (RICO) Act violations; or 3) one or more suspects faced federal charges and/or federal agents were described as being involved in the bust. If the media article mentioned an indictment only, and the arrest date could not be located, the incident was excluded from the analysis. Articles that mentioned arrests for simple drug possession (i.e., the individual was not charged with intent to distribute) and articles that described law enforcement action being driven by an offense other than illegal drug activity were also excluded from the major drug bust category.

Maps for the police posts in Baltimore (n=142), similar to police beats, were obtained from BPD. Previous criminology research has utilized comparable units of analysis to understand the impacts of various violence reduction interventions (Sherman and Rogan, 1995; Heissel et al., 2017; Weisburd et al., 2017).

Data on the police posts in which the Safe Streets sites were located, as well as the sites' dates of operation, were provided by the Baltimore City Health Department, which has overseen Safe Streets since its inception. The Safe Streets site location in Madison-Eastend was not bounded by police post borders but instead encompassed two different posts. To account for this, a composite "faux post" was created to represent the program boundaries in Madison-Eastend.

The dates and districts for the Operation Ceasefire Baltimore call-ins were obtained from the Baltimore Mayor's Office on Criminal Justice, which staffed the Ceasefire program manager position and was tasked with oversight of the intervention.

## Analytic Approaches

Each analysis in this research project was conducted separately for homicides and nonfatal shootings to isolate the effects of the interventions on two different gun violence outcomes. The rationale for this distinction between homicides and nonfatal shootings was that the motivations and intentions that led to either outcome may be dissimilar; many homicides are planned acts of violence, while nonfatal shootings are often spontaneous or not intended to be lethal (Felson and Messner, 1996). Thus, it is possible that the interventions applied in Baltimore had differential effects on these two indicators of gun violence.

<u>Aim 1</u>

<u>Measures</u>

Dependent Variables: Homicides, nonfatal shootings

Independent Variables: Major drug busts, drug possession arrests, drug trafficking/distribution arrests

Control Variables: Weapon possession arrests, post-unrest time period

The homicide, nonfatal shooting, arrest, and major drug bust locations were geo-located as points and then aggregated to the police post polygon level using a shapefile of the Baltimore Police Department's police posts (142 police posts in Baltimore). The data were then totaled for each post for each month between January 2003 and December 2017 (n=180 months per post). The outcomes of interest, homicides or nonfatal shootings, were individually coded as monthly count dependent variables. The monthly count of major drug busts was coded as a series of independent variables to measure the additive effects of the busts after one, two, three, four, five, six, nine, and twelve months. Arrests for drug possession and drug trafficking/distribution were also coded as monthly count independent variables and temporally lagged by one month (t-1) to avoid endogeneity concerns (e.g., an increase in shootings may increase enforcement in the same month). Additionally, to test for distinct effects of BPD arrests of large numbers of individuals for drug trafficking/distribution within the same month and police post, a "surge" in drug trafficking/distribution arrests was defined as 15 or more arrests within a given post and month and coded as an independent indicator variable. The one-, two-, three-, four-, five-, six-, nine-, and twelve-month temporally lagged variables for drug trafficking arrest surges were created to estimate the duration of any association between homicides or nonfatal shootings in time t following arrest surges that occurred in the previous months (t-1, t-2, t-3, etc.). To attempt to account for any influence that a focus by BPD on illegal weapon carrying may have had on gun violence, a monthly count of weapon possession arrests was included in the analysis and lagged by one month (t-1).

<u>Analytic Strategy</u>

A police post-month panel dataset was created and negative binomial regressions were run to estimate the incident rate ratio of the relationship between the interventions on monthly nonfatal shooting or homicide counts at the police post level. Negative binomial regression was used for these analyses to account for overdispersion in the outcomes of interest. Both homicides and nonfatal shootings are rare outcomes with wide variance across police posts in Baltimore, so the variances in counts across the panel dataset were much greater than the means. In negative binomial regression, monthly post level counts of homicide or nonfatal shooting counts are modeled as a function of covariates through a log-linear link function,

$$\ln(\mu_{it}) = \beta_0 + \beta_1 X_{1it} + \beta_2 X_{2it} \ldots \beta_k X_{kit},$$

where $\mu_{it}$ = the log of the count of homicides or nonfatal shootings in post i at time t; $\beta_0$ is the intercept; and $\beta_1 X_{1it} \ldots \beta_k X_{kit}$ represent the set predictor and control variables in the model. The general analytic models for each study objective were as follows:

Objective 1: Examine the spatial-temporal association of homicides or nonfatal shootings in Baltimore between 2003 and 2017 as a function of major drug busts (DRUGBUSTS), drug possession arrests (DRUGPOSS), drug trafficking arrests (DRUGTRAFF), weapon possession arrests (WPNPOSS), and drug trafficking surges (DRUGTRAFFSURGE)

$$\ln(\mu_{it}) = \beta_0 + \beta_1 DRUGBUST_{it} + \beta_2 DRUGPOSS_{it} + \beta_3 DRUGTRAFF_{it} + \beta_4 WPNPOSS_{it} + \beta_5 DRUGTRAFFSURGE_{it}$$

Objective 2: Examine whether there are differential effects of the spatial-temporal association between homicides or nonfatal shootings and major drug busts if the busts are of individuals alleged to be violent actors in the illicit drug trade (DRUGBUST_VIOLENCE).

$$\ln(\mu_{it}) = \beta_0 + \beta_1 DRUGBUST\_VIOLENCE_{it} + \beta_2 DRUGPOSS_{it} + \beta_3 DRUGTRAFF_{it} + \beta_4 WPNPOSS_{it}$$

Objective 3: Examine the spatial-temporal association of homicides or nonfatal shootings as a function of major drug busts for which at least one federal agency is involved in the busts (DRUGBUST_FEDINVOLVEMENT).

$$\ln(\mu_{it}) = \beta_0 + \beta_1 DRUGBUST\_FEDINVOLVEMENT_{it} + \beta_2 DRUGPOSS_{it} + \beta_3 DRUGTRAFF_{it} + \beta_4 WPNPOSS_{it}$$

Objective 4: Determine if the spatial-temporal association between homicides or nonfatal shootings and drug law enforcement changed following the civil unrest in April 2015 (POSTUNREST) following the in-custody death of Freddie Gray, Jr.

$$\ln(\mu_{it}) = \beta_0 + \beta_1 DRUGBUST_{it} + \beta_2 DRUGPOSS_{it} + \beta_3 DRUGTRAFF_{it} + \beta_5 WPNPOSS_{it} + \beta_5 DRUGTRAFFSURGE_{it} + \beta 6POSTUNRESTit + \beta_7 DRUGBUST{*}POSTUNREST_{it} +$$

$\beta_8$DRUGPOSS*POSTUNREST$_{it}$ + $\beta_9$DRUGTRAFF*POSTUNREST$_{it}$ +

$\beta_{10}$WPNPOSS*POSTUNREST$_{it}$ + $\beta_{11}$DRUGTRAFFSURGE*POSTUNREST$_{it}$

Generalized linear models were used with robust standard errors to specify that intragroup correlation may occur by police post. To identify and account for any potential displacement effects of law enforcement activity, spatial lag versions of the predictor and control variables (drug possession arrests, drug trafficking arrests, major drug busts, and weapon possession arrests) were included in all models. Neighboring police posts were defined as those that shared a contiguous boundary with the focal police post, with contiguity defined as "at least one point on the boundary of one polygon is within the snap distance of at least one point of its neighbour," or analogous to the "queen" move in the game chess (Bivand, 2018, p.2). A spatial lag variable was defined for each focal police post as the mean of that variable in the neighboring police posts. For example, each model considers the monthly count of drug possession arrests in the focal police post as well as the average number of drug possession arrests in the neighboring police posts.

A Durbin-Wu-Hausman test was conducted to confirm the use of fixed versus random effects due to correlation between the unobserved effects and the explanatory variables. Dummy variables for month and year were included in the models to control for seasonality and other unmeasured time-variant changes in factors. To control for post-specific trends throughout the study period that could contribute to variance in homicides or nonfatal shootings by post, a dummy variable for post was also included. All models were run with and without the corresponding spatial lag variables and tested for model fit.

Sensitivity Analyses

In addition to the analyses and objectives specified above, all analyses were considered at the city level in addition to the post level, given that certain drug law enforcement activity, particularly major drug busts, may have ripple effects beyond a small geographic area. However, the city-level analyses yielded no additional information than the post-level analyses presented in this paper and suggested that the smaller unit of analysis offers better precision of the estimates. Two other stratifications of major drug busts were explored as well. First, the differential impact of the number of suspects arrested during the drug busts was examined, assuming that the coordinated arrests of larger numbers of individuals at a time could have a more noticeable effect on their respective illicit drug markets. Also, in the media reports of drug busts, there were sometimes multiple geographic areas alleged to be illicit drug market locations tied to the arrestees. Thus, the effects of major drug busts were analyzed using two different ways of estimation: one that took into account the primary street location of the bust listed in the media article, and one that coded each geographic area (street intersection or neighborhood linked to a corresponding police post) mentioned in the news article as being impacted by the bust. The first study objective was analyzed using both the stratification by suspects per bust and the stratification by single versus multiple locations of the alleged illicit drug market activity. The initial results from those analyses generated estimates that varied little from the simpler methods of stratification presented here. Thus, those stratifications were excluded from the final models and analyses, and the analyses in this study used the primary street location of the major drug bust as the treated geographic area.

Aim 2

Measures

Dependent Variables: Homicides, nonfatal shootings

Independent Variable: The presence of the Safe Streets intervention in a police post

Control Variables: Drug possession arrests, drug trafficking arrests, weapon possession arrests

The homicide, nonfatal shooting, and arrest locations were geo-located as points and then aggregated to the police post polygon level using a shapefile of Baltimore's 142 police posts. The data were totaled for each month between January 2003 and December 2017. All police posts were then coded to delineate the respective Safe Streets posts and months in which the program was/has been in operation, taking into account periods of inactivity.

Analytic Strategy

The synthetic control method was used to estimate the effects of Safe Streets in each of the police posts where the intervention was implemented. Given the substantial heterogeneity among neighborhoods in Baltimore and the inability to directly measure factors that impact trends that vary from one neighborhood to the next, it is challenging to find fitting comparison police posts to compute the intervention's effect. The synthetic control method creates an estimate of the counterfactual for the treated police post, or a "synthetic control," that is generated from a weighted combination of comparison police posts from the donor pool, where the weights are chosen based on the comparison posts' ability to most accurately predict the pre-intervention trends in the outcome variable in the treated police post (Abadie and Gardeazabal, 2003; Abadie, Diamond and Hainmueller, 2010; Abadie, Diamond and Hainmueller, 2015). This approach creates a vector of weights that minimizes the root mean squared prediction error between the homicides or nonfatal shootings during the pre-intervention period and the weighted vector of outcomes and covariates in the control police posts during the pre-intervention period. Because the method uses data from only those police posts in the door pool that best fit the trends of the treated post prior to the intervention, it can produce a more accurate estimate of the counterfactual for the treated police post and the impact of the intervention post-implementation than analytic approaches that estimate the treatment effects on a much broader set of data, including non-intervention posts which may be substantially different from the intervention post. The synthetic control method also avoids the assumption that an intervention's effects are constant across all observations, which underlies estimates gleaned from traditional regression analyses. This methodology is appropriate for comparative case studies and allows for the separate estimation of the effects of each Safe Streets site on homicides or nonfatal shootings.

To construct the appropriate synthetic controls for each Safe Streets site and both outcomes of interest, the donor pool of comparison police posts was restricted to the 136 Baltimore City police posts that have not implemented a Safe Streets program. Annual logged averages of homicides, nonfatal shootings, and arrests for weapon possession, drug possession, and drug trafficking for each year leading up to the intervention were used as pre-intervention covariates to estimate the trend for the synthetic control prior to the implementation of Safe Streets in a particular police post. Due to the volatility of homicide or nonfatal shooting count data and to ease interpretation, the use of six-month cumulative averages, twelve-month cumulative averages, three-month moving averages, and five-month moving averages of the dependent variable was tested (Abadie, Diamond and Hainmueller 2015; Rudolph et al., 2015; Crifasi et al., 2015). The five-month averages (t-1, t-1, t, t+1, and t+2) created the most consistent fit and were a logical approach for this analysis when considering that the impact of Safe Streets would likely not be seen for a couple months following the program implementation. Homicides or nonfatal shootings that occurred during the intervention month were excluded from the pre-intervention averages.

The synthetic controls' ability to predict pre-intervention trends in homicide or nonfatal shooting rates in the Safe Streets police posts using weighted combinations of select posts over the use of all non-Safe Streets police posts was assessed by calculating the pre-intervention homicide or nonfatal shooting average in the Safe Streets post and contrasting it with the pre-intervention homicide or nonfatal shooting average for the entire pool of comparison posts.

The synthetic control method does not produce traditional tests of statistical significance, so a demonstrated way to assess the likelihood that the estimates generated by the synthetic control method are due to the interventions is to perform “in-space placebo tests” with each of the comparison units and run the analyses with each unit in the donor pool as if it received the intervention at the same time as the treated unit (Abadie, Diamond and Hainmueller 2010; Abadie, Diamon and Hainmueller 2015; Rudolph et al., 2015; Crifasi et al., 2015). Because the variance in homicides and nonfatal shootings across police posts in Baltimore is so wide, the police posts used for the placebo tests were restricted to only those 64 non-Safe Streets posts in the top 50$^{th}$ percentile for homicide and nonfatal shooting counts over the study period (2003-2017) in order to generate relatively comparative results to the posts where Safe Streets has been implemented. The percent difference in total post-implementation homicide or nonfatal shooting counts between the observed and estimated counterfactual from each synthetic control model was then calculated. This allowed for a comparison of the estimated percent change associated with the Safe Streets intervention to the percent change estimate derived from the placebo tests with the control posts in each respective donor pool. Finally, the proportion of control posts with an estimated change in homicides or nonfatal shootings that was more favorable than the percent change estimated in the Safe Streets posts was calculated. This proportion, similar to a p-value, provided an assessment of how much one can attribute the estimated percent change in homicides or nonfatal shootings in the Safe Streets posts to the interventions themselves (Rudolph et al., 2015).

<u>Aim 3</u>

<u>Measures</u>

Dependent Variables: Homicides, nonfatal shootings

Independent Variables: The presence of the Ceasefire enforcement intervention within a police district (separate for Western and Eastern Districts) and the cumulative effect of the Ceasefire call-ins within a police district (separate for Western and Eastern Districts)

Control Variables: Drug possession arrests, drug trafficking arrests, weapon possession arrests, post-unrest time period

The homicide, nonfatal shooting, and arrest locations were geo-located as points and then aggregated to the police post polygon level using a shapefile of Baltimore's 142 police posts. The data were then totaled for each post for each month between January 2003 and December 2017 (n=180 months per post). The outcomes of interest, homicides or nonfatal shootings, were individually coded as monthly count dependent variables. Each police post was assigned to its corresponding district number so that the post-level data could also be aggregated to the police district level.

Although the Ceasefire program team mapped the geographic territories or primary locations of individuals and groups identified as being the most violent in the Eastern and

Western Districts to neighborhood blocks, the individuals and groups themselves were obviously not physically constrained, so enforcement tactics, as well as the messages of deterrence and assistance, were also not tightly restricted to just a neighborhood or a police post. Thus, one independent variable, representing the time period following a Ceasefire call-in, was coded as an indicator variable, with "1" representing a post-call-in month in a police post in the Western or Eastern District and "0" otherwise. If a call-in occurred after the $15^{th}$ of the month, the indicator variable was turned on beginning the following month. An additional independent variable was created to indicate the cumulative total of the call-ins that had occurred at a given time, so that each subsequent call-in contributed to the additive effect of the intervention in the Western or Eastern District. The independent variables were coded such that there were two variables for each district.

Like the homicide and nonfatal shooting data, the drug- and weapon-related arrest data were geo-located as points onto a shapefile of Baltimore City, aggregated to the police post polygon level, and then totaled for each month of the study period. Arrest data were then coded as monthly count control variables and lagged by one month (t-1) to address endogeneity concerns (e.g., shootings can spur increased enforcement in a given area and time). The civil unrest in late April 2015 led to sharp increases in homicides and nonfatal shootings across Baltimore but particularly in the Western and Eastern Districts. Therefore, an indicator variable for the unrest was included in all models, with "0" representing the months prior to the unrest and "1" representing the months following the unrest.

Analytic Strategy

A police post-month panel dataset was created using the data described above, with each post coded to its respective police district to capture district-level effects. Negative binomial regressions were conducted to estimate the incident rate ratio of the relationship between the Ceasefire intervention and monthly nonfatal shooting or homicide counts in the Western or Districts. In negative binomial regression, monthly post level counts of homicide or nonfatal shooting counts are modeled as a function of covariates through a log-linear link function,

$$\ln(\mu_{it}) = \beta_0 + \beta_1 X_{1it} + \beta_2 X_{2it} \ldots \beta_k X_{kit}$$

where $\mu_{it}$ = the log of the count of homicides or nonfatal shootings in post i at time t; $\beta_0$ is the intercept; and $\beta_1 X_{1it} \ldots \beta_k X_{kit}$ represent the set predictor and control variables in the model. The analytic models for this analysis were as follows:

$$\ln(\mu_{it}) = \beta_0 + \beta_1 \mathrm{CEASEFIREWEST\_ENFORCEMENT}_{it} + \beta_2 \mathrm{CEASEFIREWEST\_CUMULATIVE}_{it} + \beta_3 \mathrm{CEASEFIREEAST\_ENFORCEMENT}_{it} + \beta_4 \mathrm{CEASEFIREEAST\_CUMULATIVE}_{it} + \beta_5 \mathrm{DRUGPOSS}_{it} + \beta_6 \mathrm{DRUGTRAFF}_{it} + \beta_7 \mathrm{WPNPOSS}_{it} + \beta_8 \mathrm{POSTUNREST}_{it}$$

An analysis of the aggregate Ceasefire effect was also conducted, using the following model:

$$\ln(\mu_{it}) = \beta_0 + \beta_1 \mathrm{ANYCEASEFIRE\_ENFORCEMENT}_{it} + \beta_2 \mathrm{ANYCEASEFIRE\_CUMULATIVE}_{it} + \beta_3 \mathrm{DRUGPOSS}_{it} + \beta_4 \mathrm{DRUGTRAFF}_{it} + \beta_5 \mathrm{WPNPOSS}_{it} + \beta_6 \mathrm{POSTUNREST}_{it}$$

The variables in the analytic models are defined as follows: $\mu$ = homicide or nonfatal shooting counts; i = post (n=142); t = month (n = 180); CEASEFIREWEST_ENFORCEMENT = an after-call-in month in the Western District; CEASEFIREWEST_CUMULATIVE = the number of call-ins that had occurred in the Western District in a given month; CEASEFIREEAST_ENFORCEMENT = an after-call-in month in the Eastern District; CEASEFIREEAST_CUMULATIVE = the number of call-ins that had occurred in the Eastern District in a given month; ANYCEASEFIRE_ENFORCEMENT = an after-call-in month in the

Western or Eastern District; ANYCEASEFIRE_CUMULATIVE = the number of call-ins that had occurred in the Western or Eastern District in a given month; DRUGPOSS = drug possession arrests; DRUGTRAFF = drug trafficking/distribution arrests; WPNPOSS = weapon possession arrests; POSTUNREST = the months following the civil unrest in Baltimore in late April 2015.

A Hausman test was conducted to confirm the use of fixed versus random effects due to correlation between the unobserved effects and the explanatory variables. Month- and year-fixed effects estimators were included in the models to control for seasonality and other time-variant changes in factors not measured in the models. To control for post-specific unmeasured trends throughout the study period that could contribute to variance in homicides or nonfatal shootings, a post-level fixed effects estimator was also included.

The quantitative analysis was supplemented with semi-structured, one-on-one interviews with six key personnel who were instrumentally involved in the Ceasefire intervention. A copy of the interview guide is included in Appendix C. The key informants were employees of the Baltimore Police Department, Baltimore State's Attorney's Office, or the Mayor's Office during the design, implementation, and/or execution of Ceasefire. Recruitment of the key informants was conducted based on the researcher's knowledge of the Ceasefire intervention through the researcher's employment in the Baltimore City Mayor's Office from February 2013 through November 2015. The key informants were initially contacted for the study by cell phone text, email, or Facebook Messenger. Once the informants agreed to talk by phone, they were called, provided an explanation of the study, and asked to participate. All six of the individuals contacted agreed to be interviewed. The interviews were conducted face-to-face in restaurants or office spaces. The interviewees were instructed that their responses were confidential and would not be reported in a manner that could lead to their identification. The interviews ranged from 30 to 75 minutes and were recorded on a laptop using QuickTime Player audio recording software. Notes were also taken during the interviews. After each interview, the recordings were replayed and the notes were edited to ensure the details of each conversation were documented in the notes. A grounded analysis was used to identify themes as they emerged from the interview data. The notes from the first interview were reviewed and organized into themes, which were used to create a codebook. The notes from each subsequent interview were reviewed, and words, phrases, or sentences were then organized using the codebook. New themes that emerged were added to the codebook. After reviewing all notes once, the notes were reviewed a second time to ensure that content from each interview was properly organized into the appropriate thematic category.

All geocoding of point data and aggregation to police post polygons was completed using ESRI Business Analyst 2015 software in ArcGIS Desktop 10.4.1 (ESRI, 2015). The creation of all spatial lag variables was completed using the R Statistical Computing Environment (R Core Team, 2018) with R-contributed packages for GEE-based regression inference and spatial statistical operations, including gee, rgdal, spdep, and maptools. All negative binomial regressions and synthetic control models were performed in Stata/IC 15.1 for Mac (64-bit Intel) (StataCorp, 2017). Data management was shared across all three software platforms.

This research project was deemed “not human subject research” by the Johns Hopkins Bloomberg School of Public Health.

## Aim 1: Examining the Spatial and Temporal Associations Between Drug Law Enforcement Interventions and Gun Violence in Baltimore, Maryland

### Introduction

Law enforcement agencies in the United States, as well as journalists and crime scholars, have long associated much of the violent crime in urban areas with participation in the illegal drug trade (Ousey and Lee, 2004; Goldstein, 1985). Drug law or drug prohibition enforcement, defined as “police-, military-, or forced-based responses to illicit drugs that emphasize the imposition of criminal laws for drug use and drug-related crimes” (i.e., possession, distribution, and production) (Csete et al., 2016), has become a major component of local, state, and federal drug prohibition strategies since the 1980s, when laws and sanctions became increasingly punitive for drug law violations (Courtwright, 2004). The use of drug law enforcement interventions to address community-level disorder, crime, and violence has become a standard operating procedure for many police departments across the country (Reaves, 2015). However, there have been relatively few quantitative research studies on the impact of drug law enforcement interventions on violence reduction in the United States. The available research on the relationship between drug law enforcement actions and violent crime suggests that many of the actions used by local police agencies to interrupt drug markets may have either an undetectable or violence-generating effect on local levels of violence (Werb et al., 2011; Mazerolle, Soole and Rambouts, 2006; Csete et al., 2016). Without a clearer understanding of both the real and perceived impact that drug prohibition enforcement interventions have on the communities where they are implemented, police department tactics to disrupt illicit drug markets, reduce violence, and ultimately protect and serve their communities may unintentionally contribute to quite opposite effects.

Data to examine the mechanisms and processes that influence the relationship between drug prohibition enforcement and violent crime are difficult to obtain. Based on research showing that individuals involved in the illicit drug trade sometimes engage in the use of violence to gain/maintain market share or settle disputes (Donohue and Levitt, 1998; Brownstein, Crimmins and Spunt, 2000; Blumstein, 1995), one might theorize that the removal of participants in illegal drug markets via arrest and incarceration has the potential to reduce violence and other drug-related crime. Alternatively, the disruption of drug distribution networks might instead lead to increases in violence through myriad mechanisms. Dependence on income resulting from drug sales by individuals who might have scarce opportunities to make money in the legal economy could lead to violence that is motivated by economic desire, such as robbery of other drug dealers for money or product (Miron, 1999). The complex relationship between groups, drugs, and violence also plays an important role in the manner in which disruptions in illicit drug markets could increase violence (Bellair and McNulty, 2009). Internal group disputes within drug crews over market share and unofficial contractual agreements may arise due to the destabilization of business flows (Miron, 1999). Violence may be utilized as an intimidation or elimination strategy to discourage cooperation with law enforcement agents who are targeting specific individuals in the drug trade (Reuter, 2009). Furthermore, the disruption of markets due to law enforcement intervention, via the removal of competitors, could also increase profits for other sellers, potentially inciting remaining or would-be suppliers to forcefully and violently secure the now-unattended market share (Reuter, 2009; Miron, 1999). Additionally, police pressure could unintentionally increase predatory or retaliatory behavior by leading dealers to rush transactions and thus increasing the opportunity for swindling or ambush, creating a marketplace for dealers to sell adulterated or fake product, or raising the risk of violence against perceived police informants (Jacques and Allen, 2014).

The association between the illicit drug market and violent crime has helped to justify the substantial increase in the late 1980s and 1990s in resource allocation and commitment to drug control policies by local, state, and federal law enforcement agencies nationwide (Sacco, 2014). Billions of dollars are spent in the United States each year at the local, state, and federal levels on domestic enforcement of drug control policies (Office of National Drug Control Policy, 2015), yet there are few existing published studies that have quantitatively examined the impact that drug law enforcement in the United States has had on decreasing supply, consumption, or violence associated with illicit drug markets. These studies have analyzed various proxy measures for drug law enforcement, such as rates or counts of drug arrests, number of police officers assigned to drug law enforcement initiatives, or resource allocation in the police departments' budget for drug-related interventions, on their impact on violent crime rates. Most of the published analyses have found that drug law enforcement actually leads to increases in violence.

A 2006 meta-analytical review of street drug law enforcement examined evaluations of several interventions launched, managed, and/or implemented by local law enforcement to prevent or reduce illegal drug use, illegal drug selling, and associated problems in drug selling locations (Mazerolle, Soole and Rambouts, 2006). The researchers assessed pre- to post-intervention effects on a number of outcome variables, including drug- and nondrug-related calls for service, property crimes, and violent offenses, and found that of the reviewed studies that measured impact on reported crimes against persons or calls for service for violent crimes demonstrated significant impact. A 2011 systematic literature review to assess the relationship between drug law enforcement approaches and drug market-related violence reduction yielded eleven U.S.-based studies that employed longitudinal data analyses of empirical data (Werb et al., 2011). Nine of the eleven quantitative evaluations reviewed reported a significantly positive (harmful) relationship between drug law enforcement and drug market violence. For instance, an in-depth economic analysis on financial activities of a drug-dealing street gang in Chicago found that the lack of official dispute settlement channels and pressure from drug law enforcement contributed to high levels of violence. A longitudinal observational study of 67 counties in Florida and found that drug enforcement measures such as resource allocation, number of sworn officers, and number of drug arrests and convictions were positively correlated with Part I violent crimes (Benson, 1998). An examination of violent crime rates to the proportion of drug arrests to total arrests in 24 United States Metropolitan Statistical Areas (MSAs) in 1992 and 1993 revealed a significant and positive association between the two rates (Resignato, 2000).

Looking beyond the United States, there is also evidence that drug prohibition enforcement may contribute to violent crime. The Johns Hopkins Lancet-Commission on Drug Policy and Health reviewed existing literature on public health issues stemming from drug policy and found that much of the drug-related violence worldwide is associated with the protection of illicit drug markets by drug cartels against armed government or paramilitary forces. The researchers also highlighted that “some experts have suggested that heavy crackdowns by drug police can lead to major increases in violence when disruption of a criminal network leads rival groups to intensify their efforts to capture the territory of the weakened group" (Csete et al., 2016, p. 7), using Mexico and Central America as examples. Mexico has experienced a historic rise in homicides in since 2006, unprecedented for any country not formally involved in war. This dramatic increase in homicide has been linked to the government's decision to utilize the military to fight drug traffickers in civilian areas (Heinle, Ferreira and Shirk, 2015). Other research found that approximately 25% of Colombia's homicide rate between 1994 and 2008 could be explained by law and militarized enforcement of drug laws surrounding the illicit cocaine trade, and that homicides spiked when drug law enforcement was most intense (Mejia and Restrepo, 2014). While one can recognize that there are substantial differences between the United States and Mexico or Colombia, given the findings in existing literature, the potential correlations between drug law enforcement tactics and violence associated with the illicit drug trade in this country cannot be ignored.

The existing peer-reviewed research on the relationship between law enforcement-led drug market disruptions and violent crime in the United States are ptimarily based on data from the 1980s, 1990s and very early 2000s. Furthermore, the majority have either been descriptive studies in one city (Goldstein et al., 1997) or cross-sectional analyses across multiple localities (Rasmussen, Benson and Sollars, 1993; Benson, 2001), so they do not allow for estimations of trends over time. Also, given that illicit drug markets may be geographically localized – potentially operating only within specific sections of a given neighborhood – it is important to conduct research on the effects of drug law enforcement on violence at the smallest geographic unit of analysis possible.

The current study expands the literature examining the impact of drug law enforcement on gun violence in the United States by analyzing changes over space and time in counts of homicides or nonfatal shootings in Baltimore, Maryland, following interventions used by the Baltimore Police Department to explicitly enforce drug prohibition laws. The study objectives were as follows:

1. Examine the spatial-temporal association of drug law enforcement interventions – drug possession arrests, drug trafficking/distribution arrests, and major drug busts – and homicides or nonfatal shootings in Baltimore.
2. Examine whether there are differential effects of the spatial-temporal association between major drug busts and gun violence if the arrests in those busts are of individuals alleged to be violent actors in the illicit drug trade.
3. Examine whether the spatial-temporal association between major drug busts and gun violence in Baltimore differs based on the involvement of at least one federal agency in the arrests.
4. Determine if the spatial-temporal association between drug law enforcement in Baltimore and gun violence changed following the civil unrest in April 2015 after the in-custody death of Freddie Gray, Jr.

## Methods

### Setting

The study was conducted in Baltimore, Maryland, a city that has had consistently high rates of both gun violence and illicit drug use for decades.  In 2014, Baltimore had the sixth highest rate of violence among American cities with populations greater than 250,000 people (United States Department of Justice, 2014).  Additionally, Baltimore has one of the highest per capita rates of injection drug use in the United States (Friedman et. al, 2004), and targeting illicit drug markets to curb violence in the city has long been a priority strategy for local, state, and federal law enforcement.

## Study Design

A multiple interrupted time-series study design was used to examine the association between drug law enforcement interventions employed by the Baltimore Police Department – specifically, drug possession arrests, drug trafficking arrests, and major drug busts - and homicides or nonfatal shootings in the months following those interventions. The interrupted time-series design is appropriate for studies of individual treatment groups that are repeatedly measured over time to assess an outcome of interest before and after an intervention (Biglan, Ary and Wagenaar, 2000). This design allows for an evaluation of the various interventions' associations with the outcomes of interest: homicides or nonfatal shooting counts in each of the 142 police posts (precincts) in Baltimore.

## Data Sources

The outcomes of interest for this study were homicide or nonfatal shooting counts, as they are the most reliably tracked incidents of violence and the majority of homicides committed in Baltimore involve firearms; over 84% of all Baltimoreans killed between 2012 and 2017 died by gun violence (Baltimore Police Department, 2018). The Baltimore Police Department (BPD) directly provided individual-level information on all homicides and nonfatal shootings between January 1, 2003, and November 30, 2015. The homicide and nonfatal shooting data from BPD included the date and physical location of the incident, as well as the victim's date of birth, gender, and age. Data for homicides and nonfatal shootings occurring between December 1, 2015, and December 31, 2017, were obtained through the city of Baltimore's Open Data catalog for BPD Part I Victim Based Crime Data (Open Baltimore, 2018). These data are updated weekly by BPD and, in addition to the incident date, include the street block, weapon type, premise, and XY coordinates of each reported crime incident, but they do not include any victim-specific information.

To measure BPD's enforcement of drug law prohibitions, individual-level arrests for drug possession and drug trafficking or distribution violations between January 1, 2003, and December 31, 2015, were obtained directly from BPD. To attempt to account for any influence that BPD's utilization of proactive policing and prioritization of illegal weapon carrying may have had on homicide or nonfatal shooting counts, data on weapon possession arrests for the same time period was also obtained from BPD. The drug- and weapon-related arrest data only included the arrest date, time, location, and arrest charge(s). Drug possession, drug trafficking/distribution, and weapon possession arrests from January 1, 2016, through December 31, 2017, were obtained by downloading all arrest data from Baltimore's Open Data catalog for BPD Arrests (Open Baltimore, 2018) and then using relevant key words to extract arrests for drug possession, drug trafficking/distribution and weapon possession. The Open Baltimore database is updated with individual-level arrest data weekly by the Baltimore Police Department and includes the arrestee's gender and race, in addition to the arrest date, time, street block location, XY coordinates of the arrest, and arrest charge. The online database includes a statement that the data on the site represent the most serious arrest charge of an individual processed at Baltimore's Central Booking and Intake Facility and that arrests for individuals who were processed at Baltimore's Juvenile Booking Facility are excluded from the site. This limitation suggests that some arrest data that could be relevant to the analysis may not be included in the Open Baltimore database. However, a comparison by count and arrest type (drug possession, drug trafficking, or weapon possession) of Open Baltimore arrest data from January 1, 2012, through December 31, 2015, with the corresponding arrest data provided directly by BPD yielded a net 95% match between the two datasets, so the Open Baltimore data was chosen as a suitable substitute for this analysis.

To measurer larger-scale, more resource-intensive and coordinated drug law enforcement activity, information was extracted from articles from *The Baltimore Sun*, the city's largest print media source, that detailed arrests and indictments for illegal drug sales between January 1, 2003, and December 31, 2017. A ProQuest Central search was conducted using the Baltimore Sun publication identification number (46036) and the following keywords: ("drug" OR "narcotic" OR "cocaine" OR "heroin" OR "marijuana") AND ("arrest" OR "prosecution" OR "prosecute" OR "indict" OR "indictment"). The ProQuest Central search yielded 3,693 articles, of which 809 were reviewed. Any event documented in a *Baltimore Sun* article that mentioned an arrest and/or indictment of individuals for drug law violations as a "bust." The ProQuest Central article search was supported by an article search in Google, using the same keywords as above but adding "Baltimore" and each year of the study period in separate searches, to ensure that no major drug-related arrests were missed. A drug bust was categorized as "major" if any of the following conditions were met: 1) five or more individuals were arrested in the bust; 2) charges included drug conspiracy, drug kingpin statute, running a violent drug gang, continuing a criminal enterprise, or Racketeer Influenced and Corrupt Organizations (RICO) Act violations; or 3) one or more suspects faced federal charges and/or federal agents were described as being involved in the bust. If the media article mentioned an indictment only, and the arrest date could not be located, the incident was excluded from the analysis. Articles that mentioned arrests for simple drug possession (i.e., individual was not charged with intent to distribute) and articles that described law enforcement action being driven by an offense other than illegal drug activity were also excluded from the major drug bust category.

Measures

The dependent variables for this study were homicide or nonfatal shootings. The explanatory variables were drug busts, drug possession arrests, drug trafficking/distribution arrests. Weapon possession arrests, as well as a variable indicating the months following the civil unrest, were used as control variables. The homicide, nonfatal shooting, arrest, and major drug bust locations were geo-located as points and then aggregated to the police post polygon level using a shapefile of BPD's police posts (142 police posts in Baltimore). The data were then totaled for each post for each month between January 2003 and December 2017 (n=180 months per post). The outcomes of interest, homicides or nonfatal shootings, were individually coded as monthly count dependent variables. The monthly count of major drug busts was coded as a series of independent variables to measure the additive effects of the busts after one, two, three, four, five, six, nine, and twelve months. Arrests for drug possession and drug trafficking/distribution were also coded as monthly count independent variables and temporally lagged by one month (t-1) to avoid endogeneity concerns (e.g., an increase in shootings may increase enforcement in the same month). Additionally, to test for distinct effects of BPD arrests of large numbers of individuals for drug trafficking/distribution within the same month and police post, a "surge" in drug trafficking/distribution arrests was defined as 15 or more arrests within a given post and month and coded as an independent indicator variable. The one-, two-, three-, four-, five-, six-, nine-, and twelve-month temporally lagged variables for drug trafficking arrest surges were created to estimate the duration of any association between homicides or nonfatal shootings in time *t* following arrest surges that occurred in the previous months (t-1, t-2, t-3, etc.). To attempt to account for any influence that a focus by BPD on illegal weapon may have had on gun violence, a monthly count of weapon possession arrests was included in the analysis and lagged by one month (t-1).

Analytic Strategy

A police post-month panel dataset was created and regressions were run using generalized linear models with a negative binomial distribution. Robust standard errors were specified to account for intragroup correlation that may occur by police post. A Durbin-Wu-Hausman test was conducted to confirm the use of fixed versus random effects due to correlation between the unobserved effects and the explanatory variables. Dummy variables for month and year were included in the models to control for seasonality and other unmeasured time-variant changes in factors. To control for post-specific trends throughout the study period that could contribute to variances in homicides or nonfatal shootings by post, a dummy variable for post was also included.

To identify and account for any potential displacement effects of law enforcement activity, spatial lag versions of the predictor and control variables (drug possession arrests, drug trafficking arrests, major drug busts, and weapon possession arrests) were included in all models. Neighboring police posts were defined as those that shared a contiguous boundary with the focal police post, with contiguity defined as “at least one point on the boundary of one polygon is within the snap distance of at least one point of its neighbour,” or analogous to the “queen” move in the game chess (Bivand, 2018, p.2). A spatial lag variable was defined for each focal police post as the mean of that variable in the neighboring police posts. For example, each model considers the monthly count of drug possession arrests in the focal police post as well as the average number of drug possession arrests in the neighboring police posts.

All models were run with and without the corresponding spatial lag variables and tested for model fit. The spatial lag variables generally did not improve model fit or suggest spillover effects of violence and were excluded from the final models; exceptions are detailed in the Results section. Estimates were exponentiated and are presented as incident rate ratios with associated p-values; asterisks delineate the estimates that were significant at the $p<0.05$, $p<0.01$, and $p<0.001$.

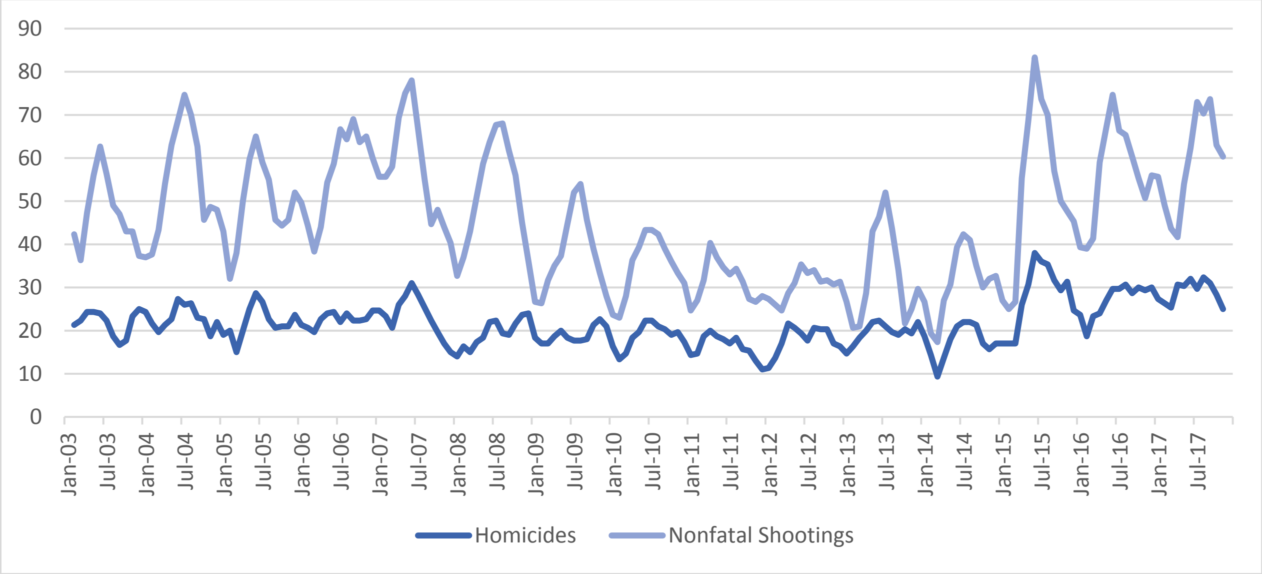
All geocoding of point data and aggregation to police post polygons was completed using ESRI Business Analyst 2015 software in ArcGIS Desktop 10.4.1 (ESRI, 2015). The creation of all spatial lag variables was completed using the R Statistical Computing Environment (R Core Team, 2018) with R-contributed packages for GEE-based regression inference and spatial statistical operations, including gee, rgdal, spdep, and maptools. All negative binomial regressions were performed in Stata/IC 15.1 for Mac (64-bit Intel) (StataCorp, 2017). Data management was shared across all three software platforms.

This study was deemed “not human subject research” by the Johns Hopkins Bloomberg School of Public Health.

**Results**

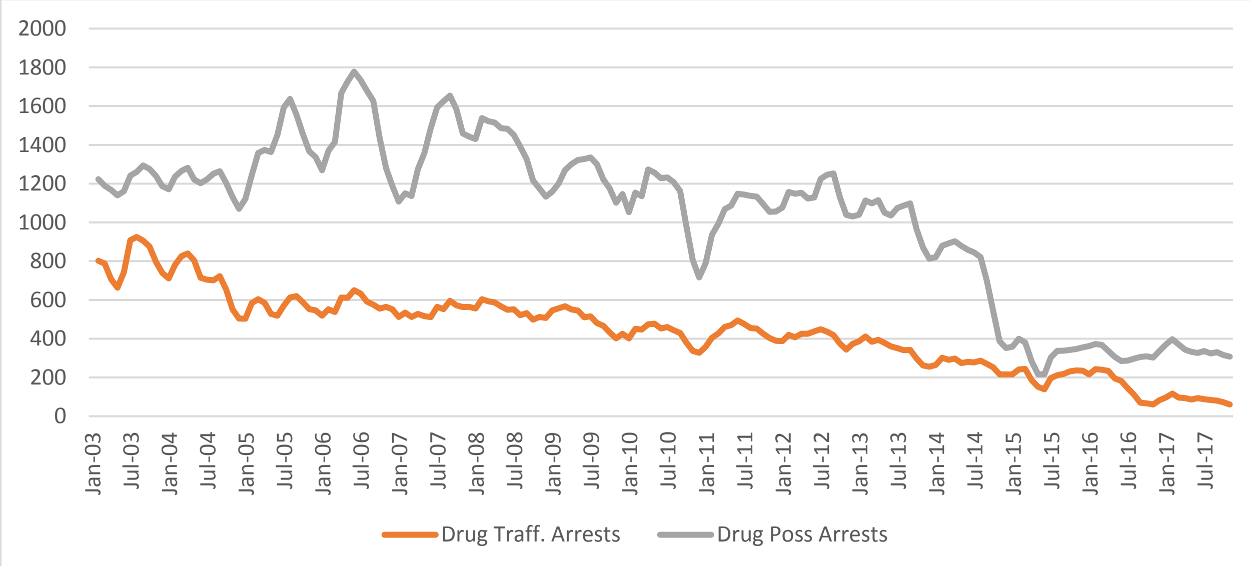
During the study period of January 2003 through December 2017, there were 3,936 homicides and 8,246 nonfatal shootings recorded by the Baltimore Police Department. Three-month moving averages for these outcomes are presented in Figure 2.1.

Figure 2.1: Three-Month Moving Averages, Homicides & Nonfatal Shooting Counts, 2003-2017




BPD also recorded 198,056 drug possession arrests, 84,748 drug trafficking arrests, and 17,571 weapon possession arrests during the same time period. The three-month moving averages for drug possession and drug trafficking/distribution arrest counts are illustrated in Figure 2.2. While the number of drug-related arrests steadily decreased over the study period, and drug possession arrests dropped dramatically after the state of Maryland decriminalized possession of small amounts of marijuana in 2014, the number of arrests for simple weapon possession remained relatively stable, with the exception of a spike in weapon possession arrests for about one year following the civil unrest that occurred in Baltimore in late April 2015 (see Appendix A).

Figure 2.2: Three-Month Moving Averages, Drug Possession & Drug Trafficking Arrests, 2003-2017




The geo-location of the homicide, nonfatal shooting, and arrest data resulted in the retention of 98.9% of homicides, 99.2% of nonfatal shootings, 93.6% of drug possession arrests, 93.0% of drug trafficking arrests, and 90.7% of weapon possession arrests.

Out of 160 total drug busts in Baltimore between 2003 and 2017 that were reported in online articles from the Baltimore Sun and other local media outlets for this study period, 72 were determined to be major drug busts in Baltimore City. However, details on the respective arrest locations, arrest years, and/or arrest months were unavailable for 28 busts, leaving 44 major drug busts for inclusion in this study. There were 815 drug trafficking arrest surges during the study period.

Table 2.1 presents the results from the first set of negative binomial regressions. Estimates for the association between major drug busts and homicides suggested a harmful relationship, while the association between major drug busts and nonfatal shootings suggested a protective relationship, though none of the estimates for either outcome of interest were statistically significant. No clear pattern of association between drug trafficking arrest surges and homicides was found. Drug possession arrests in the police post were the arrests took place were associated with slight increases in nonfatal shootings in the following month (Incident Rate Ratio (IRR): 1.005, p=0.043), while the spatial lag (SL) for drug possession arrests suggested that an increase in drug possession arrests in neighboring police posts was associated with a decrease in nonfatal shootings in the main post in the following month (IRR: 0.986, p=0.043). Drug trafficking arrest surges were associated with a 16-24% increase in nonfatal shootings in the three months (IRR: 1.237, p=0.002), four months (IRR: 1.164, p=0.051), and five months (IRR: 1.242, p=0.015) following those surges.

Table 2.1: Regression Results, Major Drug Bust and Drug Trafficking Arrest Surge Models, IRRs (p-values)

| | Major Drug Bust Model | | | | Drug Trafficking Arrest Surge Model | | | |
|---|---|---|---|---|---|---|---|---|
| | Homicides | | Nonfatal Shootings | | Homicides | | Nonfatal Shootings | |
| Drug Poss Arrests | 0.999 | (0.860) | 1.005* | (0.043) | 0.999 | (0.838) | 1.005* | (0.044) |
| Drug Poss Arrests SL | 0.997 | (0.609) | 0.986*** | (0.001) | 0.997 | (0.564) | 0.985*** | (0.001) |
| Drug Traff Arrests | 0.998 | (0.699) | 0.998 | (0.709) | 1.003 | (0.616) | 1.004 | (0.529) |
| Weapon Poss Arrests | 1.000 | (0.974) | 0.996 | (0.731) | 1.000 | (0.986) | 0.994 | (0.659) |
| Drug Bust, 1-mo effect | 1.747 | (0.166) | 0.913 | (0.680) | | | | |
| Drug Bust, 2-mo effect | 1.032 | (0.908) | 0.796 | (0.168) | | | | |
| Drug Bust, 3-mo effect | 1.019 | (0.922) | 0.777 | (0.069) | | | | |
| Drug Bust, 4-mo effect | 1.162 | (0.313) | 0.866 | (0.260) | | | | |
| Drug Bust, 5-mo effect | 1.136 | (0.304) | 0.903 | (0.337) | | | | |
| Drug Bust, 6-mo effect | 1.141 | (0.218) | 0.875 | (0.198) | | | | |
| Drug Bust, 9-mo effect | 1.094 | (0.318) | 0.898 | (0.212) | | | | |
| Drug Bust, 12-mo effect | 1.055 | (0.502) | 0.880 | (0.080) | | | | |
| Drug Traff Surges, 1-mo lag | | | | | 0.868 | (0.247) | 0.850 | (0.122) |
| Drug Traff Surges, 2-mo lag | | | | | 0.876 | (0.215) | 1.084 | (0.313) |
| Drug Traff Surges, 3-mo lag | | | | | 1.080 | (0.448) | 1.237** | (0.002) |
| Drug Traff Surges, 4-mo lag | | | | | 0.987 | (0.892) | 1.164 | (0.051) |
| Drug Traff Surges, 5-mo lag | | | | | 1.050 | (0.617) | 1.242* | (0.015) |
| Drug Traff Surges, 6-mo lag | | | | | 0.962 | (0.627) | 1.070 | (0.397) |
| Drug Traff Surges, 9-mo lag | | | | | 0.916 | (0.429) | 1.118 | (0.139) |
| Drug Traff Surges, 12-mo lag | | | | | 1.139 | (0.071) | 1.126 | (0.124) |

Exponentiated coefficients; p-values in parentheses
* p<0.05, ** p<0.01, *** p<0.001

The second analysis considered whether the relationship between major drug busts and homicides and or nonfatal shootings might change if the individuals arrested in those busts were alleged to be violent actors in the illicit drug trade. While it is believed by law enforcement that many of the individuals arrested in major drug busts are involved either directly or indirectly in illicit drug market violence, this analysis concentrated on specific mentions in news articles of the arrested individuals' alleged ties to homicides or nonfatal shootings in the city. There were 13 major drug busts in Baltimore City for which the media articles made explicit note of the arrested individuals' or groups' ties to homicides or nonfatal shootings; 10 of those 13 busts could be geo-located and were included in the analysis.

There was no statistically significant association between homicides and major drug busts with or without explicit, alleged ties to homicides or nonfatal shootings (Table 2.2). On the other hand, taking into account major drug busts of individuals who *were not* linked to alleged homicides or nonfatal shootings, major drug busts of individuals who *were* alleged to be connected to homicides or nonfatal shootings were associated with a 52% reduction in nonfatal shootings after four months, (IRR: 0.481, p=0.045), a 49% reduction in nonfatal shootings after five months (IRR: 0.507, p=0.018), and a 47% reduction in nonfatal shootings after six months (IRR: 0.533, p=0.011).

Table 2.2: Regression Results, Major Drug Busts Stratified by Explicit Ties to Violence, IRRs (p-values)

| | Homicides | | Nonfatal Shootings | |
|---|---|---|---|---|
| Drug Poss Arrests | 0.999 | (0.861) | 1.005* | (0.043) |
| Drug Poss Arrests SL | 0.997 | (0.608) | 0.986*** | (0.001) |
| Drug Traff Arrests | 0.998 | (0.696) | 0.998 | (0.733) |
| Weapon Poss Arrests | 1.001 | (0.973) | 0.996 | (0.732) |
| Drug Bust, 1-mo effect w/ violence | 1.985 | (0.298) | 0.142 | (0.183) |
| Drug Bust, 1-mo effect w/o violence | 1.665 | (0.293) | 1.221 | (0.331) |
| Drug Bust, 2-mo effect w/ violence | 1.400 | (0.496) | 0.373 | (0.062) |
| Drug Bust, 2-mo effect w/o violence | 0.916 | (0.788) | 0.940 | (0.717) |
| Drug Bust, 3-mo effect w/ violence | 1.287 | (0.497) | 0.504 | (0.102) |
| Drug Bust, 3-mo effect w/o violence | 0.936 | (0.792) | 0.868 | (0.340) |
| Drug Bust, 4-mo effect w/ violence | 1.067 | (0.840) | 0.481* | (0.045) |
| Drug Bust, 4-mo effect w/o violence | 1.192 | (0.336) | 0.991 | (0.947) |
| Drug Bust, 5-mo effect w/ violence | 0.988 | (0.961) | 0.507* | (0.018) |
| Drug Bust, 5-mo effect w/o violence | 1.182 | (0.266) | 1.029 | (0.811) |
| Drug Bust, 6-mo effect w/ violence | 0.999 | (0.997) | 0.533* | (0.011) |
| Drug Bust, 6-mo effect w/o violence | 1.183 | (0.166) | 0.980 | (0.865) |
| Drug Bust, 9-mo effect w/ violence | 1.035 | (0.897) | 0.717 | (0.070) |
| Drug Bust, 9-mo effect w/o violence | 1.112 | (0.279) | 0.954 | (0.613) |
| Drug Bust, 12-mo effect w/ violence | 1.054 | (0.804) | 0.832 | (0.162) |
| Drug Bust, 12-mo effect w/o violence | 1.055 | (0.586) | 0.895 | (0.179) |

Exponentiated coefficients; p-values in parentheses

* $p<0.05$, ** $p<0.01$, *** $p<0.001$

There were 22 of 44 major drug busts in this study for which at least one federal agency shared credit - the Bureau of Alcohol, Tobacco, Firearms, and Explosives, the Drug Enforcement Agency, the Federal Bureau of Investigation, or United States Immigration and Customs Enforcement. No significant associations were found between homicides or nonfatal shootings and major drug busts stratified by federal agency involvement; estimates are available in Appendix A.

The final analysis considered whether the civil unrest in April 2015 following the in-custody death of Freddie Gray, Jr. changed the magnitude or direction of the association between drug law enforcement and homicides or nonfatal shootings. Table 2.3 details the total numbers of homicides, nonfatal shootings, arrests, and major drug busts that occurred before and after the unrest.

Table 2.3: Total Events Included in Study, Pre- and Post-Civil Unrest in April 2015

| | Before/During Civil Unrest | After Civil Unrest |
|---|---|---|
| Homicide Victims | 2,964 | 932 |
| Nonfatal Shooting Victims | 6,291 | 1,888 |
| Drug Possession Arrests | 174,842 | 10,455 |
| Drug Trafficking Arrests | 74,544 | 4,280 |
| Weapon Possession Arrests | 13,106 | 2,832 |
| Drug Trafficking Arrest Surges | 801 | 14 |
| Major Drug Busts | 31 | 13 |

Two separate models were used to test the associations between major drug busts and drug trafficking arrest surges and the dependent variables, though the results are presented together in Table 2.4. The inclusion of spatial lag variables improved the fit for both models but were overall nonsignificant and did not add value to the analysis, so with the exception of the drug possession arrests spatial lag, the lag estimates for these models are only presented in Appendix A. For homicides, the only significant estimate was for the "post unrest" indicator variable (in the major drug bust models, IRR: 1.499, p=0.015; in the drug trafficking arrest surges model, IRR: 1.470, p=0.022). Estimates for the major drug busts prior to the unrest, at any temporal duration, were in the harmful direction but nonsignificant. Estimates for major drug busts following the unrest were generally in the protective direction, but the estimates were not significant. There was no consistent pattern for the association between drug trafficking arrest surges and homicide, before or after the civil unrest.

The regression analyses for nonfatal shootings, on the other hand, yielded several interesting findings. First, as expected, the "post unrest" indicator variable showed that shootings increased over 71% following the unrest. Also, the associations between drug possession arrests and nonfatal shootings appeared to only hold prior to the unrest, as the estimates for "post unrest" interacted with drug possession arrests or the lag variable for drug possession arrests were no longer significant. The control variable, weapon possession arrests, was associated with a 6% increase in nonfatal shootings in the months following their execution (IRR: 1.063, p=0.016). The associations between major drug busts and nonfatal shootings prior to or following the unrest appeared to be protective after the first month following the busts, although none of the estimates were significant in the analysis. In models examining the differential temporal effects of drug trafficking arrest surges, the arrest surges prior to the unrest were associated with a 16-27% increase in nonfatal shootings in the three to five months following their execution. No clear pattern of association between nonfatal shootings and drug trafficking arrest surges after the unrest could be ascertained, given that the statistically significant estimates varied greatly in direction and magnitude.

Table 2.4: Results for Pre- and Post-Unrest Analyses, Major Drug Bust and Drug Trafficking Arrest Surge Models, IRRs (p-values)

| | Major Drug Bust Model | | | | Drug Trafficking Arrest Surge Model | | | |
|---|---|---|---|---|---|---|---|---|
| | Homicides | | Nonfatal Shootings | | Homicides | | Nonfatal Shootings | |
| Drug Poss Arrests | 0.999 | (0.714) | 1.005* | (0.039) | 0.999 | (0.725) | 1.005* | (0.043) |
| Drug Poss Arrests SL | 1.000 | (0.955) | 0.987** | (0.005) | 1.001 | (0.918) | 0.986** | (0.003) |
| Drug Traff Arrests | 1.001 | (0.916) | 0.998 | (0.769) | 1.005 | (0.440) | 1.004 | (0.560) |
| Weapon Poss Arrests | 1.003 | (0.859) | 0.980 | (0.145) | 1.002 | (0.903) | 0.979 | (0.120) |
| Drug Bust, 1-mo effect | 2.013 | (0.200) | 1.182 | (0.481) | | | | |
| Drug Bust, 2-mo effect | 1.014 | (0.974) | 0.868 | (0.315) | | | | |
| Drug Bust, 3-mo effect | 1.038 | (0.897) | 0.802 | (0.061) | | | | |
| Drug Bust, 4-mo effect | 1.191 | (0.460) | 0.860 | (0.304) | | | | |
| Drug Bust, 5-mo effect | 1.134 | (0.564) | 0.894 | (0.409) | | | | |
| Drug Bust, 6-mo effect | 1.056 | (0.784) | 0.864 | (0.279) | | | | |
| Drug Bust, 9-mo effect | 1.075 | (0.591) | 0.947 | (0.599) | | | | |
| Drug Bust, 12-mo effect | 1.070 | (0.560) | 0.925 | (0.380) | | | | |
| Drug Traff Surges, 1-mo lag | | | | | 0.869 | 0.275) | 0.863 | (0.174) |
| Drug Traff Surges, 2-mo lag | | | | | 0.884 | (0.234) | 1.077 | (0.347) |
| Drug Traff Surges, 3-mo lag | | | | | 1.111 | (0.312) | 1.232** | (0.004) |
| Drug Traff Surges, 4-mo lag | | | | | 0.970 | (0.742) | 1.163 | (0.053) |
| Drug Traff Surges, 5-mo lag | | | | | 1.091 | (0.378) | 1.269** | (0.008) |
| Drug Traff Surges, 6-mo lag | | | | | 0.939 | (0.480) | 1.014 | (0.872) |
| Drug Traff Surges, 9-mo lag | | | | | 0.903 | (0.377) | 1.134 | (0.106) |
| Drug Traff Surges, 12-mo lag | | | | | 1.106 | (0.198) | 1.145 | (0.094) |
| Post-Unrest | 1.499* | (0.015) | 1.717*** | (<0.001) | 1.470* | (0.022) | 1.712*** | (<0.001) |
| Drug Poss*Unrest | 1.019 | (0.156) | 0.997 | (0.784) | 1.019 | (0.156) | 0.997 | (0.804) |
| Drug Poss*Unrest SL | 1.026 | (0.292) | 0.998 | (0.940) | 1.024 | (0.337) | 1.000 | (0.998) |
| Drug Traff*Unrest | 1.001 | (0.960) | 1.009 | (0.667) | 1.005 | (0.801) | 1.014 | (0.587) |
| Weapon Poss*Unrest | 0.971 | (0.362) | 1.063* | (0.016) | 0.970 | (0.347) | 1.062* | (0.019) |
| Drug Bust, 1-mo effect*Unrest | 0.635 | (0.516) | 0.359 | (0.169) | | | | |
| Drug Bust, 2-mo effect*Unrest | 1.051 | (0.935) | 0.739 | (0.580) | | | | |
| Drug Bust, 3-mo effect*Unrest | 0.946 | (0.921) | 0.885 | (0.745) | | | | |
| Drug Bust, 4-mo effect*Unrest | 0.921 | (0.861) | 0.985 | (0.960) | | | | |
| Drug Bust, 5-mo effect*Unrest | 0.995 | (0.990) | 0.984 | (0.943) | | | | |
| Drug Bust, 6-mo effect*Unrest | 1.228 | (0.486) | 0.982 | (0.933) | | | | |
| Drug Bust, 9-mo effect*Unrest | 1.067 | (0.719) | 0.818 | (0.303) | | | | |
| Drug Bust, 12-mo effect*Unrest | 0.972 | (0.887) | 0.823 | (0.293) | | | | |
| Drug Traff Surges, 1-mo lag*Unrest | | | | | 0.626 | (0.308) | 0.555 | (0.065) |
| Drug Traff Surges, 2-mo lag*Unrest | | | | | 1.850 | (0.063) | 1.191 | (0.680) |
| Drug Traff Surges, 3-mo lag*Unrest | | | | | 0.546 | (0.091) | 1.054 | (0.857) |
| Drug Traff Surges, 4-mo lag*Unrest | | | | | 1.476 | (0.314) | 1.439 | (0.354) |
| Drug Traff Surges, 5-mo lag*Unrest | | | | | 0.208 | (0.194) | 0.412*** | (<0.001) |
| Drug Traff Surges, 6-mo lag*Unrest | | | | | 1.935 | (0.087) | 3.024*** | (<0.001) |
| Drug Traff Surges, 9-mo lag*Unrest | | | | | 1.402 | (0.330) | 1.027 | (0.929) |
| Drug Traff Surges, 12-mo lag*Unrest | | | | | 1.116 | (0.708) | 0.798 | (0.313) |

Exponentiated coefficients; p-values in parentheses

* p<0.05, ** p<0.01, *** p<0.001

**Discussion**

This research sought to understand the relationship between drug law enforcement and gun violence by considering how small- and large-scale arrests of individuals involved in the illicit drug trade may lead to changes in homicide or nonfatal shooting rates in the areas where the arrests or drug activity occurred. The analyses presented in this study suggest that drug possession arrests, drug trafficking arrests, and major drug busts, regardless of the presence of federal agency involvement or the potential apprehension of violent individuals in those busts, did not led to an appreciable reduction in homicides in Baltimore over the fifteen-year study period. Major drug busts overall also did not lead to reductions in nonfatal shootings. There is evidence that major drug busts of individuals who were allegedly and explicitly linked to violence may have some protective effect on nonfatal shootings, but this needs to be further explored with future research, given that most estimates were nonsignificant and this analysis only found protective effects when comparing those busts to busts of individuals not explicitly linked to violence. Drug possession arrests, as well as drug trafficking/distribution arrest surges, were associated with statistically significant increases in nonfatal shootings in the months following their occurrences. The estimates for the spatial lag variable for drug possession arrests suggest potential displacement of illicit drug activity following the arrests. Many individuals who are arrested for drug possession may also be low-level drug sellers, as the user and seller populations often overlap (Floyd et al., 2010; Vaughn et al., 2015). Thus, one theory to explain the potential displacement is that arrests of individuals in one neighborhood leads to sellers in bordering areas moving in to supply the now-unattended market, and this migration of sellers translates into less violence in the areas from which the sellers came. However, additional research is needed to further understand the dynamics related to illicit drug activity following the arrests of individuals for drug possession and to determine if there is support for this theory.

Weapon possession arrests in the post-unrest period were associated with increases in nonfatal shootings in the one month following their occurrences. This finding is difficult to interpret; although one might theorize that an arrest of a group member for weapon possession could sometimes cause concern regarding law enforcement cooperation and could potentially lead to intra-group disputes or even violence, it is hard to comprehend how the arrest of individuals for weapon possession would consistently lead to increases in shootings in the following month.

The findings from this study are congruent with the existing literature on the relationship between drug law enforcement and violence in cities in the United States and countries around the world. These results suggest that drug law enforcement has little, if any, protective impact on gun violence, and is more likely to instead be associated with increased violence in the neighborhoods where the law enforcement interventions occur. The Drug Market Intervention (DMI), or "pulling levers" policing, is a strategy first implemented in High Point, North Carolina, to reduce crime and violence associated with open-air drug markets (Corsaro et al., 2012). The intervention combines various components of other strategies found to be effective at reducing various types of crime, such as problem-solving policing, focused deterrence, targeted enforcement, reconciliation between law enforcement and communities, and community engagement. Large reductions in drug market-related crime and violence were attributed to the DMI strategy in High Point; however, most replications of the intervention in other cities have not yielded similar reductions in violent crime and, similar to the findings in this study, the intervention has at times been associated with increases in violence instead (Braga, Weisburd and Turchan, 2018; Webster and Buggs, 2017). Thus, little evidence supports the notion that drug law enforcement, even with incorporated elements of successful crime-reduction approaches, can lead to decreased gun violence. The market forces that affect the demand for illicit drugs, as well as the potential profits to be made, especially for those who have lack alternative economic opportunities, appear to be overwhelmingly difficult to overcome through the threats of incarceration and sanctions that come with the enforcement of drug prohibition.

The finding that major drug busts focused on individuals believed to be involved in violence may lead to better outcomes than busts of those not explicitly linked to violence is also consistent with existing literature on focused deterrence, which devotes law enforcement and community resources to identifying and apprehending violent, group-involved individuals (Kennedy, 2006). The focused deterrence approach to group violence reduction has been found to have significant reductions in violent crime, even in communities with high rates of drug activity (Braga, Weisburd and Turchan, 2018). Concentrating law enforcement efforts on individuals most responsible for violence and coordinating their strategies with community organizations and residents is far more likely to help achieve desired public safety gains than continuing to direct enforcement on drug law prohibitions.

The limitations of this research are important to acknowledge. The use of police arrest data as a measure of the extent and location of illicit drug markets is limited in that it reflects the actions of the police department itself, rather than capturing an accurate measure of drug market-related activity. Lagged effects of the drug law enforcement interventions were included in this study to help minimize concerns of endogeneity. Another issue with the use of police arrest data is that they do not capture federal drug-related arrests, as those arrested individuals are not processed through the same system as those arrested by the Baltimore Police Department. There is not a reliable database of federal drug-related arrests, but future research in this area would ideally include those arrests in the analyses.

There are also limitations of the use of media reports to account for the details of major drug busts, as it is possible that the number of busts included in this study is an underestimate of the total major drug busts over the study period. However, the use of print or online media to document events is a common way to utilize publicly available data for research, and it seems logical that the arrests of major actors in the illegal drug trade, particularly those believed to be tied to violence, would be documented in the media and emphasized to the public as accomplishments by law enforcement. An additional limitation of this project is that there are assumptions made about the appropriate geographic unit of analysis. Gun violence is more likely to occur at the street or block level, and thus the aggregation of data to the police post level can result in loss of precision. However, prior research on gun and drug market-related violence has utilized police beats or police posts as the unit of analysis, primarily since this is the smallest unit for which law enforcement-specific incident-level data are available, and effect sizes have been detected at this unit in numerous studies, including this one.

This study also has several strengths. This research is only one of two known studies to use a longitudinal data analysis to estimate a spatial-temporal association between drug law enforcement interventions and gun violence, and it is the only study that explored potential differential effects of major drug busts due to the involvement of federal agents or the alleged connections to violence among the arrestees. The study relied on study designs and analytic methods used reliably in previous research on drug market-related violence and/or the concentration of violent victimization among vulnerable populations. It also will help shed important light on how drug law enforcement interventions may impact the public safety, and thus the health and well-being, of communities impacted by high rates of illicit drug activity and violent crime.

Policymakers at the local, state, and federal levels must seriously consider the harms of drug control policy versus its benefits. By continuing to invest heavily in drug law enforcement strategies that are not increasing public safety, they are undercutting efforts and investments that could provide the public with effective and sustainable public safety strategies. There must be candid discussions about the true costs of current drug law enforcement policies and the implementation of new strategies that will improve the quality of life for all citizens and communities impacted by illicit drug markets without imposing harm to communities, especially those that are already plagued with disparate rates of community violence.

## Aim 2: Evaluating the effects of Safe Streets Baltimore on gun violence

### Introduction

City leaders in Baltimore, Maryland, have long grappled with how to address the city's stubbornly high rates of gun violence. In 2007, the city introduced a community-based public health intervention that had demonstrated success in reducing homicides and nonfatal shootings in other cities in the United States (Webster et al., 2013). Modeled after the CeaseFire (now called Cure Violence) program in Chicago, the intervention was designed to reduce the incidence of violence, change behaviors of youth at high risk of violence perpetration by providing alternative methods of issue resolution and increasing the perceived risks and costs of involvement in violence, and change social and community norms about the acceptability of using guns to resolve conflict. In Baltimore, the program was implemented under the name Safe Streets. Since its inception in 2007, Safe Streets has been implemented in a total of seven neighborhoods over the past decade, although as of June 2018, the program was in operation in only four neighborhoods. An early evaluation of Safe Streets found that the program was associated with statistically significant reductions in homicides and/or non-fatal shootings in three of the four neighborhoods where it was implemented, as well as in neighboring communities (Webster et al., 2013). However, the researchers found that the intervention effects were inconsistent across sites and noted that further reductions in gun violence might have been possible with stronger program implementation and greater fidelity to the original Chicago model. Since the original evaluation of the Safe Streets program, Safe Streets was expanded to three additional neighborhoods in Baltimore, yet no formal evaluation of the program's impact on gun violence incidents has since been conducted. The purpose of this study was to build on the earlier analysis of Safe Streets' impact on gun violence and examine the effectiveness of the program in all of the neighborhoods where it has existed.

## The Cure Violence Model

Cure Violence is a multi-component, community-level intervention that employs street outreach workers to develop relationships and engage with individuals at high risk for committing or being victims of violence. The program model entails three key components: interrupting transmission of violence by mediating conflicts and limiting the likelihood of retaliation; identifying those at highest risk of perpetration of violence and reducing their risk through behavior change and linkage to needed services; and changing community norms around violence through community organization and anti-violence messaging (Cure Violence, 2018). The outreach workers often themselves have had extensive criminal histories, previous incarcerations, and/or former gang affiliations and are generally well known in the communities in which they work. Being formerly engaged in and familiar with the very behaviors and activities they hope to change increases the likelihood that the outreach workers will be seen by their intended clients as credible messengers and thus potentially trustworthy resources. The outreach staff serve as role models who can exhibit prosocial behavior while also helping to link the individuals to critical supports and services (educational, financial, health, job training, etc.). Through the relationships built by the outreach workers and the connections to systems and supports that can help address the needs of clients and their families, these at-risk individuals will ideally choose positive paths of development and conflict resolution.

The program also employs special outreach workers who operate primarily as violence interrupters, working to identify, resolve and de-escalate potentially dangerous conflicts that could lead to shootings. These staff play an essential part in working with individuals with high risk of violence perpetration to teach them how to resolve conflict and situations that elicit negative affect without resorting to lethal violence, recognizing that while the program cannot always intervene at the initial act of violence, it can minimize the spread of violence by interrupting that transmission through conflict mediation. The third key component of the intervention involves community mobilization and addressing social norms that perpetuate violence. The program staff help to organize responses to homicide and nonfatal shooting incidents and engage with community partners to promote anti-violence messages and an intolerance for using guns to resolve conflicts, often through public events or social campaigns (Butts et al., 2015).

The Cure Violence program model has been implemented and evaluated in numerous cities around the United States. Impact studies of the program have found mixed results of the intervention's success at reducing gun homicides and nonfatal shootings (Butts et al., 2015; Cerdá, Tracy and Keyes, 2017). For example, in Chicago, an interrupted time series analysis of the program found that the program was associated with 16-28% reductions in nonfatal shootings in four of seven Cure Violence communities and had varying impact across sites on outcomes such as gang involvement in homicide and retaliatory shootings by gang members (Skogan et al., 2009). An independent evaluation of Philadelphia's Cure Violence program found that the intervention led to a statistically significant 30% reduction in nonfatal shootings after two years (Cure Violence, 2018), while an analysis of a program based upon the Cure Violence model in Phoenix, Arizona, found that the intervention was actually associated with a significant increase in nonfatal shootings and a significant decrease in assaults (Fox et al., 2015).

Several evaluations of Cure Violence model replications have also examined the program's influence on social and community norms about gun violence. These studies found that the intervention positively influenced youth's willingness to use violence to settle conflict (Delgado, Alsabahi and Butts, 2017) and increased confidence in the intervention's engagement with community around antiviolence messaging (Picard-Fritsche and Cerniglia, 2013).

Safe Streets in Baltimore, Maryland

In 2007, the Baltimore City Health Department received a $1.6 million grant from the U.S. Department of Justice to implement the Cure Violence program model in Baltimore (Webster et al., 2013). The program, named Safe Streets, serves youth ages 14 to 25 who are at the highest risk of perpetrating violence and living in communities that experience high rates of gun homicides and nonfatal shootings. Safe Streets was initiated in single police posts within four neighborhoods – McElderry Park, Elwood Park, Madison-Eastend, and Cherry Hill - between 2007 and 2008. A fifth site was planned in a police post in the Union Square neighborhood but encountered substantial implementation challenges and was terminated within one year. The Safe Streets program expanded to Baltimore's Mondawmin community in 2012 and Lower Park Heights in 2013. Following the civil unrest that occurred following the death of Freddie Gray, Jr., in April 2015, and the sharp increase in gun violence across the city but particularly in West Baltimore, city officials opened a Safe Streets site in the Sandtown-Winchester community in 2016.

Researchers analyzed Safe Streets' impact on gun violence several years after the program was implemented in Baltimore. Using a difference-in-differences analytic strategy to compare homicide and nonfatal shooting incident rates in intervention sites to rates in bordering communities and other communities with high rates of violence, and controlling for law enforcement activities and arrests, the researchers found that only the Cherry Hill program was associated with significant reductions in both homicides (-56%) and nonfatal shootings (-34%)

(Webster et al., 2013), while the other sites had reductions in either homicides or nonfatal shootings but not both.

Researchers have also examined the Safe Streets program's influence on youth's attitudes about the acceptability of the use of guns to settle conflicts (Milam et al., 2016a) and found that respondents in Lower Park Heights had significant improvements in attitudes towards violence and a greater magnitude of improvement in violent attitudes to personal conflict than did those in the comparison community. (Milam et al, 2016b). Additionally, the presence of anti-violence signs and interactions with Safe Streets workers led to significant increases in nonviolent attitudes toward conflict.

The Johns Hopkins Center for Gun Policy and Research recently examined various interventions, including Safe Streets, used in Baltimore to reduce homicides or nonfatal shootings (Webster, Buggs and Crifasi, 2018). Using negative binomial regressions and controlling for law enforcement activities, baseline differences in homicide and nonfatal shooting rates, and the citywide impact of the April 2015 civil unrest, the researchers found no aggregate effects of all Safe Streets sites on homicides from 2007-2017. Models that estimated site-specific effects of Safe Streets found that only Cherry Hill experienced a statistically significant homicide reduction (-45%) from program inception to the period of civil unrest. The Safe Streets sites in Lower Park Heights and McElderry Park were associated with slight, nonsignificant reductions in homicides prior to the unrest, while the Mondawmin site was associated with an unfavorable but nonsignificant increase in homicides. Analyzing the program's impact on nonfatal shootings, the researchers found only a small but nonsignificant reduction when aggregating the effects across all Safe Streets sites. The site-specific analyses yielded no statistically significant reductions, although the estimates for Cherry Hill, showing a

30% reduction in nonfatal shootings prior to the civil unrest and a 34% reduction following the unrest, did approach significance. The directions of the program effects for the other Safe Streets sites varied widely.

This research study was designed to further examine the effectiveness of Safe Streets on reducing gun violence using an increasingly popular analytic approach, the synthetic control method, to more precisely estimate the impact of Safe Streets in the neighborhoods where it has been introduced.

## Methods

### Setting

The Safe Streets program has been implemented in a total of seven police posts in Baltimore neighborhoods since 2007. The communities were chosen due to their high rates of homicides and nonfatal shootings; each police post was in the top 25$^{th}$ percentile of homicides and shootings in the city. The site locations and dates of operation are listed in Table 4.1.

Table 4.1: Safe Streets Locations and Operation Dates

| **Neighborhood** | **Dates of Operation** |
|---|---|
| McElderry Park | July 2007 – July 2015; September 2015 – present |
| Elwood Park | March 2008 – July 2010 |
| Madison-Eastend | November 2008 – June 2010 |
| Cherry Hill | January 2009 – present |
| Mondawmin | July 2012 – December 2013; January 2014 – June 2016 |
| Lower Park Heights | June 2013 – present |
| Sandtown | March 2016 – present |

Study Design

This study used a quasi-experimental research design with monthly, police-post level counts of homicides or nonfatal shootings to contrast differences before and after the implementation of Safe Streets in the seven neighborhoods in Baltimore where the intervention has been in effect compared with police posts that did not have a Safe Streets site. The synthetic control method allowed for a single-jurisdiction analysis to compare pre- and post-intervention outcomes to outcomes over the same time period in a weighted combination of comparison jurisdictions.

Data Sources

The outcomes of interest for this study were homicide or nonfatal shooting counts, as they are the most reliably tracked incidents of violence and the majority of homicides committed in Baltimore involve firearms; over 84% of all Baltimoreans killed in the past six years died by gun violence (Baltimore Police Department). The Baltimore Police Department (BPD) directly provided individual-level information on all homicides and nonfatal shootings between January 1, 2003, and November 30, 2015. The homicide and nonfatal shooting data from BPD included the date and physical location of the incident, as well as the victim's date of birth, gender, and age. Data for homicides and nonfatal shootings occurring between December 1, 2015, and December 31, 2017, were obtained through the city of Baltimore's Open Data catalog for BPD Part I Victim Based Crime Data (Open Baltimore, 2018). These data are updated weekly by BPD and, in addition to the incident date, include the street block, weapon type, premise, and XY coordinates of each reported crime incident, but they do not include any victim-specific information.

Data on the police posts in which the Safe Streets sites were located, as well as the sites' dates of operation, were provided by the Baltimore City Health Department, which has overseen Safe Streets since its inception. The Safe Streets site location in Madison-Eastend was not bounded by police post borders but instead encompassed two different posts. To account for this, a composite "faux post" was created to represent the program boundaries in Madison-Eastend.

To control for law enforcement activity aimed at suppressing gun violence, data on individual-level arrests for drug possession, drug trafficking or distribution, or illegal weapon possession violations were obtained directly from BPD for the years 2003-2015 and from Baltimore's Open Data catalog for BPD Arrests (Open Baltimore, 2018) for years 2016-2017.

## Measures

The dependent variables for this study were homicide or nonfatal shootings. The independent variable was the presence of the Safe Streets intervention within a police post. Drug possession arrests, drug trafficking arrests, and weapon possession arrests were included in the synthetic control models to help estimate the pre-intervention trends in homicides or nonfatal shootings in the police post where the Safe Streets program was implemented, given that high rates of drug and weapon possession arrests in Baltimore correlate with high rates of violence.

The homicide, nonfatal shooting, and arrest locations were geo-located as points onto a shapefile of Baltimore City and then aggregated to the police post polygon level using a shapefile of the Baltimore Police Department's police posts. The data were totaled for each month between January 2003 and December 2017 (n=180). All police posts were then coded to delineate the respective Safe Streets posts and months in which the program was/has been in operation. These data were used to create a post-month panel dataset for the synthetic control analysis.

Analytic Strategy

The synthetic control method was used to estimate the effects of Safe Streets in each of the police posts where the intervention was implemented. Given the substantial heterogeneity among neighborhoods in Baltimore and the inability to directly measure factors that impact trends that vary from one neighborhood to the next, it is challenging to find fitting comparison police posts to compute the intervention's effect. The synthetic control method creates an estimate of the counterfactual for the treated police post, or a "synthetic control," that is generated from a weighted combination of comparison police posts from the donor pool, where the weights are chosen based on the comparison posts' ability to most accurately predict the pre-intervention trends in the outcome variable in the treated police post (Abadie and Gardeazabal, 2003; Abadie, Diamond and Hainmueller, 2010; Abadie, Diamond and Hainmueller, 2015). This approach creates a vector of weights that minimizes the root mean squared prediction error between the homicides or nonfatal shootings during the pre-intervention period and the weighted vector of outcomes and covariates in the control police posts during the pre-intervention period. Because the method uses data from only those police posts in the door pool that best fit the trends of the treated post prior to the intervention, it can produce a more accurate estimate of the counterfactual for the treated police post and the impact of the intervention post-implementation than analytic approaches that estimate the treatment effects on a much broader set of data, including non-intervention comparison posts that may be substantially different from the intervention post. The synthetic control method also avoids the assumption that an intervention's effects are constant across all observations, which underlies estimates gleaned from traditional regression analyses. This methodology is appropriate for comparative case studies and allows for the separate estimation of the effects of each Safe Streets site on homicides or nonfatal shootings.

To construct the appropriate synthetic controls for each Safes Street site and both outcomes of interest, the donor pool of comparison police posts was restricted to the 136 Baltimore City police posts that have not implemented a Safe Streets program. Annual lagged averages of homicides, nonfatal shootings, and arrests for weapon possession, drug possession, and drug trafficking for each year leading up to the intervention were used as pre-intervention covariates to estimate the trend for the synthetic control prior to the implementation of Safe Streets in a particular police post. Due to the volatility of homicide or nonfatal shooting count data and to ease interpretation, the use of six-month cumulative averages, twelve-month cumulative averages, three-month moving averages, and five-month moving averages of the dependent variable was tested (Abadie, Diamond and Hainmueller 2015; Rudolph et al., 2015; Crifasi et al., 2015). The five-month averages (t-1, t-1, t, t+1, and t+2) created the most consistent fit and were a logical approach for this analysis when considering that the impact of Safe Streets would likely not be seen for a couple months following the program implementation. Homicides or nonfatal shootings that occurred during the intervention month were excluded from the pre-intervention averages.

The synthetic control method does not produce traditional tests of statistical significance, so a demonstrated way to assess the likelihood that the estimates generated by the synthetic control method are due to the interventions is to perform “in-space placebo tests” with each of the comparison units and run the analyses with each unit in the donor pool as if it received the intervention at the same time as the treated unit (Abadie, Diamond and Hainmueller 2010; Abadie, Diamon and Hainmueller 2015; Rudolph et al., 2015; Crifasi et al., 2015). Because the variance in homicides and nonfatal shootings across police posts in Baltimore is so wide, the police posts used for the placebo tests were restricted to only those 64 posts in the top 50th percentile for homicide and nonfatal shooting counts over the study period (2003-2017) in order to generate relatively comparative results to the posts where Safe Streets has been implemented. The percent difference in total post-implementation homicide or nonfatal shooting counts between the observed and estimated counterfactual from each synthetic control model was then calculated. This allowed for a comparison of the estimated percent change associated with the Safe Streets intervention to the percent change estimate derived from the placebo tests with the control posts in each respective donor pool. Finally, the proportion of control posts with an estimated change in homicides or nonfatal shootings that was more favorable than the percent change estimated in the Safe Streets posts was calculated. This proportion, similar to a p-value, provided an assessment of how much one can attribute the estimated percent change in homicides or nonfatal shootings in the Safe Streets posts to the interventions themselves (Rudolph et al., 2015).

All geocoding of point data and aggregation to police post polygons was completed using ESRI Business Analyst 2015 software in ArcGIS Desktop 10.4.1 (ESRI, 2015). All data management and synthetic control analyses were performed in Stata/IC 15.1 for Mac (64-bit Intel) (StataCorp, 2017). This study was deemed to be "not human subjects research" by the Johns Hopkins Bloomberg School of Public Health Institutional Review Board.

**Results**

Synthetic control models, first for homicides and then nonfatal shootings, were run for each of the seven Safe Streets police posts. Table 4.2 shows the root mean squared prediction error

(RMSPE) for each of the models. Each RSMPE was relatively low, which traditionally suggests that the synthetic control fits well with the pre-intervention outcome trend in the treated unit.

Table 4.2: Root Mean Square Prediction Error (RMSPE) for Pre-Intervention Period in Safe Streets Sites

| Safe Streets Sites | Homicides | Nonfatal Shootings |
|---|---|---|
| McElderry Park | 0.290 | 0.451 |
| Ellwood Park | 0.226 | 0.526 |
| Madison-Eastend | 0.253 | 0.639 |
| Cherry Hill | 0.237 | 0.596 |
| Lower Park Heights | 0.173 | 0.306 |
| Mondawmin | 0.163 | 0.314 |
| Sandtown | 0.150 | 0.310 |

In prior research, the RMSPE has been compared to a simple average of the RMSPE for all units in the donor pool (Rudolph et al., 2015; Crifasi et al., 2015). This comparison provides a logical way to demonstrating model fit when estimating the effects of policy changes in geographic areas containing large populations of people and using county- or state-level rates. However, due to the rarity of both homicides and nonfatal shootings in a much smaller unit of analysis, such as police posts, there is less confidence that the RMSPE is providing useful information about the ability of the synthetic control to better predict the pre-intervention trends in the outcome variable than using all data from all of the police posts in the control pool. Therefore, Table 4.3 lists the pre-intervention averages of the outcome in both the Safe Streets police posts and the police posts in the donor pool. This table illustrates how different the homicide or nonfatal shooting levels in each of the areas where Safe Streets was implemented were in comparison to all of the other police posts in Baltimore. For example, prior to the implementation of Safe Streets, McElderry Park was experiencing 2.4 times the average number of nonfatal shootings compared to the areas in Baltimore that did not receive the Safe Streets intervention (0.87 vs. 0.36 per month). These differences in the pre-intervention homicide or nonfatal shooting averages in the Safe Streets posts compared to the non-Safe Streets control posts support the use of the synthetic control to create a combination of weighted averages of predictors from select control units to estimate the pre-intervention outcome trend, versus the use of traditional regression analyses that take into account data from all control units, even those that are dissimilar to the treated unit.

Table 4.3: Pre-Intervention Homicide and Nonfatal Shooting Averages in Safe Streets or Control Posts

| | McElderry Park | Control Posts |
|---|---|---|
| Homicides | 0.26 | 0.15 |
| Nonfatal Shootings | 0.87 | 0.36 |
| | Elwood Park | Control Posts |
| Homicides | 0.36 | 0.15 |
| Nonfatal Shootings | 1.02 | 0.35 |
| | Madison-Eastend | Control Posts |
| Homicides | 0.21 | 0.15 |
| Nonfatal Shootings | 0.87 | 0.35 |
| | Cherry Hill | Control Posts |
| Homicides | 0.40 | 0.15 |
| Nonfatal Shootings | 0.92 | 0.35 |
| | Lower Park Heights | Control Posts |
| Homicides | 0.16 | 0.14 |
| Nonfatal Shootings | 0.46 | 0.30 |
| | Mondawmin | Control Posts |
| Homicides | 0.16 | 0.14 |
| Nonfatal Shootings | 0.35 | 0.30 |
| | Sandtown | Control Posts |
| Homicides | 0.17 | 0.14 |
| Nonfatal Shootings | 0.44 | 0.29 |

The panels of the synthetic control analyses for homicides and nonfatal shootings in each Safe Streets police post, as well as the values of the predictors in each treated police post and its respective synthetic control for the pre-intervention period and the non-zero weighted police posts that contributed to each synthetic control, are available in Appendix B. Table 4.4 lists the estimated effects of Safes Streets by site, shown as percent increases or decreases in homicides or nonfatal shootings post-program implementation. The effects of the program varied widely by both site and outcome. To test the significance of the effects, as described in the Methods section, placebo tests were run for each of the Safe Streets synthetic control models using the 64 control posts in the top 50$^{th}$ percentile for homicides and nonfatal shootings. The range of the percent changes in homicides or nonfatal shootings derived from the control police posts in the placebo tests is presented in Table 4.4. The proportion of control posts that had more favorable percent changes, or “better outcomes” in the placebo permutation tests, is also shown in Table 4.4. Based on the proportion estimates, only the 36% decrease in homicides in McElderry Park was approaching significance and can be considered a confident estimate of the program’s violence-reducing effects.

Table 4.4: Estimated Safe Streets Program Effects, Range of Percent Change in Control Posts in Placebo Tests, and Proportion of Control Posts with Better Outcomes in Placebo Tests than the Safe Streets Posts

| | Estimated Program Effect | Range of % Change in Control Posts | Proportion of Control Posts with Better Outcomes |
|---|---|---|---|
| McElderry Park | | | |
| Homicides | -36.28% | (-53.65, 100.69) | 0.06 |
| Nonfatal Shootings | 18.97% | (-61.43, 86.32) | 0.69 |
| Elwood Park | | | |
| Homicides | 95.59% | (-82.95, 338.72) | 0.94 |
| Nonfatal Shootings | -3.59% | (-87.62, 82.47) | 0.50 |
| Madison-Eastend | | | |
| Homicides | 78.56% | (-100.00, 555.00) | 0.92 |
| Nonfatal Shootings | 96.19% | (-95.83, 166.99) | 0.92 |
| Cherry Hill | | | |
| Homicides | -28.78% | (-52.14, 94.74) | 0.17 |
| Nonfatal Shootings | -4.65% | (-55.28, 129.90) | 0.38 |
| Lower Park Heights | | | |
| Homicides | -15.79% | (-66.19, 99.83) | 0.41 |
| Nonfatal Shootings | -19.05% | (-64.60, 108.47) | 0.25 |
| Mondawmin | | | |
| Homicides | 75.00% | (-67.48, 131.08) | 0.98 |
| Nonfatal Shootings | 40.84% | (-55.37, 102.15) | 0.77 |
| Sandtown | | | |
| Homicides | -9.93% | (-82.16, 119.57) | 0.50 |
| Nonfatal Shootings | 12.45% | (-76.27, 215.09) | 0.61 |

One assumption of the synthetic control method is that there are no unmeasured changes in the post-intervention period that might confound the relationship between independent and dependent variables (Rudolph et al., 2015). However, one major change that occurred in Baltimore following the implementation of six of the seven Safe Streets sites was the unrest in April 2015 and the citywide increase in homicides and nonfatal shootings that has occurred since that time. The synthetic control method does not allow for more than one change at a time, so to examine how the unrest may have impacted the estimated effects of Safe Streets in the four sites that were in operation pre- and post-unrest - McElderry Park, Cherry Hill, Lower Park Heights, and Mondawmin -, additional synthetic control analyses were conducted with a truncated post-intervention period so that the analyses ended in March 2015. Table 4.5 displays the estimated program effects for the four sites for the pre-unrest time period and the full study period. The analyses suggest that the unrest had differential impacts on the Safe Streets sites. For example, compared to the pre-unrest estimate, Lower Park Heights appears to have had a reduction in nonfatal shootings following the unrest, while Mondawmin saw dramatic increases in both homicides and nonfatal shootings following the unrest. However, none of the findings were significant when compared to the proportion of control posts with better outcomes in the placebo tests.

Table 4.5: Estimated Safe Streets Program Effects for Pre-Unrest and Full Study Periods (Proportion of Control Posts with Better Outcomes)

| | Pre-Unrest | Full Study Period |
|---|---|---|
| McElderry Park | | |
| Homicides | -31.38% (0.12) | -36.28% (0.06) |
| Nonfatal Shootings | 39.80% (0.92) | 18.97% (0.69) |
| Cherry Hill | | |
| Homicides | -23.13% (0.33) | -28.78% (0.17) |
| Nonfatal Shootings | -23.66% (0.27) | -4.65% (0.38) |
| Lower Park Heights | | |
| Homicides | -9.99% (0.50) | -15.79% (0.41) |
| Nonfatal Shootings | 16.30% (0.64) | -19.05% (0.25) |
| Mondawmin | | |
| Homicides | 32.35% (0.78) | 75.00% (0.98) |
| Nonfatal Shootings | -5.98% (0.52) | 40.84% (0.77) |

A related concern regarding the use of the synthetic control is the assumption that the impact of the intervention remains constant over the examined post-intervention period. Prior research has suggested that the ability of the synthetic control model to accurately forecast counterfactual post-intervention trends for state- or country-level policy changes may be limited to approximately ten years (Abadie, Diamond and Hainmueller, 2010; Abadie, Diamond and Hainmueller, 2015; Rudolph et al., 2015). However, it is possible that the effects of a more local-level intervention such as Safe Streets may decrease over an even shorter time span, due to changes in leadership, program staff, and unmeasured neighborhood-level factors. Thus, for the two longest-running sites, McElderry Park and Cherry Hill, varying post-intervention duration periods were considered. Table 4.6 shows the three-, five-, and seven-year program effect estimates for these two sites. When comparing the site-specific estimated effects to the proportion of controls in the placebo tests with better outcomes, only the homicide reduction in McElderry Park after three years approached significance (0.078).

Table 4.6: Three-, Five-, and Seven-Year Estimated Effects for McElderry Park and Cherry Hill (Proportion of Control Posts with Better Outcomes)

| | 3-Year Effect | 5-Year Effect | 7-Year Effect |
|---|---|---|---|
| McElderry Park | | | |
| Homicides | -64.20% (0.08) | -51.04% (0.11) | -31.25% (0.17) |
| Nonfatal Shootings | 52.37% (0.27) | 32.21% (0.81) | 39.46% (0.91) |
| Cherry Hill | | | |
| Homicides | -23.81% (0.31) | -17.68% (0.36) | -28.45% (0.16) |
| Nonfatal Shootings | -0.83% (0.53) | -2.76% (0.51) | -24.33% (0.27) |

**Discussion**

The 2012 evaluation of Safe Streets found that the program led to statistically significant reductions in homicides and/or nonfatal shootings in three of the four areas where the program had been implemented. A more recent analysis of Safe Streets' program effects suggested that the beneficial effects of the intervention had lessened over time and found statistically significant reductions in homicides only in Cherry Hill. This study sought to more closely examine the effects of Safe Streets using a statistical approach that allows for a comparison of observed homicide and nonfatal shooting rates after the program's implementation to the rates we would have expected if the program had not been implemented. The synthetic control analyses found that only one Safe Streets site – McElderry Park - had reductions in homicides that approached significance over the course of the study period. Two sites that have been shut down, Mondawmin and Madison-Eastend, experienced dramatic increases in both homicides and nonfatal shootings after the sites opened, with Mondawmin's largest increases occurring after the unrest. An examination of program effect attenuation revealed uneven effects as well, with the three-year effect of McElderry Park's program on homicide reduction approaching significance after three years but diminishing over time. The findings in this study are not incongruent with existing literature, which suggests that replications of the Cure Violence model have yielded inconsistent program-related reductions in homicides and nonfatal shootings. Recent evaluations of programs in Philadelphia and New York have found encouraging evidence of the program's ability to effectively reduce gun violence, but taken in aggregate, evaluations of the program model have shown that the protective effects have differed across space and time, as seen in this study.

This research project did not explore factors that could explain the attenuation of Safe Streets' impact on gun violence across time. The 2018 evaluation by the Johns Hopkins Center for Gun Policy and Research noted various implementation and operational challenges in a number of the Safe Streets sites, as well as geographic, social, and economic factors that may help or hinder program success in the various Baltimore communities. The authors also highlighted that the successful programs in New York City and Philadelphia have been supported by both the mayor's offices and foundations in those cities. Importantly, those programs have been strengthened with financial resources for their staff and clients, as well as wraparound services for the individuals engaged by outreach workers. Future research in this area should closely examine the components of successful replications of the Cure Violence model to better understand how factors such as worker salaries, number and type of services available to program participants, collaboration with community-based organizations, and program oversight may explain discrepancies in program impact. Additionally, it is possible that differences within neighborhood-level behaviors may affect how outreach work and conflict mediation take place. For example, a cross-sectional study of conflict mediation records in Baltimore found that program-associated reductions in homicides were associated with a higher proportion of gang-related conflict mediations, while neighborhoods without similar program-associated homicide reductions saw more weapons and retaliatory conflicts (Whitehill, Webster and Vernick, 2013). Thus, additional analysis of norms and behaviors within the neighborhoods where Safe Streets is or will operate may allow for a more appropriately tailored approach to conflict mediation and violence interruption.

One limitation of this study is the varying amount of observation data available to estimate either the pre-intervention outcome trends or the post-intervention program effects. The synthetic control models for Safe Streets in Mondawmin and Sandtown, which began in 2013 and 2016, respectively, had better model fit (per their RMSPEs) than did sites such as McElderry Park and Cherry Hill. Conversely, the earlier sites benefited from much more post-intervention data than did the sites that opened more recently. There was no appealing strategy for addressing this limitation. However, none of the RMPEs were found to be large, and the examination of program effects over different time periods, although the findings were not significant, provided insight into variance across sites over time, irrespective of program length. Another limitation is that the synthetic control model is unable to account for breaks in the intervention, such as the program suspensions in McElderry Park and Mondawmin. The suspensions were of short duration and thus likely did not have a major impact on the program's overall effect, but the breaks could nonetheless be incorporated into the models. Similarly, the program effects following the civil unrest in April 2015, which was a known shock to many neighborhoods in Baltimore, could not be isolated. Future research will further examine whether Safe Streets had protective effects following the unrest. An additional limitation of the study is that covariates used to estimate the counterfactual in the synthetic control models did not include socio-demographic characteristics. However, communities across Baltimore with high rates of gun violence, like in other urban cities in the United States, have similar socio-economic and socio-demographic characteristics, such as high residential segregation, low economic mobility, and high rates of criminal justice contact. Therefore, it is unlikely that additional demographic or economic data would improve model fit.

This study is the first known of its kind to examine site-specific effects of the Cure Violence program model on gun violence using the synthetic control method, which has clear advantages for estimating impact over traditional regression analyses. The study also considered differential program effects over time, offering insight into the challenges faced by policymakers and program leaders following program implementation. The promising findings in evaluations of the Cure Violence model in New York and Philadelphia, as well as the evidence of beneficial program effects in McElderry Park in Baltimore, suggest that the program would greatly benefit from increased resources and operational support, in addition to stronger connections to services for program participants. The Safe Streets program has just recently transitioned from the Baltimore City Health Department to the Mayor's Office. As plans to expand the program to additional neighborhoods are considered, discussions about the disparate impacts of the program to-date, action plans for increasing support, and deeper understanding about the operations of successful program in other cities should take place to ensure that it can achieve optimal outcomes.

## Aim 3: Understanding the impact of Operation Ceasefire Baltimore on gun violence through quantitative analysis and key informant interviews

### Introduction

City leaders in Baltimore, Maryland, have long grappled with how to address the city's stubbornly high rates of gun violence. Though the city continues to search for effective, long-term plans for reducing gun crime, Baltimore has experienced its lowest rates of homicides and nonfatal shootings when law enforcement prioritized the identification and apprehension of the most violent individuals, rather than using a broader approach that includes directing significant resources and manpower towards nonviolent lawbreakers (Nuckols, 2010). This strategy, known broadly as focused deterrence, has been found to help curb gun violence in cities around the world, consistently yielding moderate to substantial reductions (Braga, Weisburd and Turchan, 2018). As a program model, focused deterrence seeks to deter gun violence by homing in on those individuals believed to be involved in gun violence and applying pressure via legal and social levers. The intervention also offers to provide assistance by way of social services to individuals who want to stop participating in violence.

In 2014, Baltimore city leadership implemented its own focused deterrence program called Operation Ceasefire Baltimore (Ceasefire). Advised by David Kennedy, a criminologist who developed and popularized the focused deterrence model, Ceasefire initially claimed major reductions in gun violence in the area where it was applied. However, the program was quietly ended in 2017 and had virtually ceased all operations well before the Mayor's Office confirmed in June 2017 that the program had ended (Rodricks, 2017,). The purpose of this study was to evaluate the effect of Ceasefire on gun violence in Baltimore and provide understanding into how a violence reduction program with demonstrated success in other cities was terminated in Baltimore, particularly during a time when the city is experiencing historic rates of gun violence.

## The Focused Deterrence Model

Focused deterrence, also known as Group Violence Intervention (GVI) or "pulling levers" policing, is a violence reduction strategy designed to identify and reach individuals and groups believed to be violent actors in communities with high rates of gun violence. The intervention is supported by research showing that a substantial amount of the violence in a given community or city is committed by a small percentage of individuals, and thus directing resources and enforcement efforts toward those individuals will yield considerable community- or citywide reductions in violence.

The focused deterrence program model is comprised of the following key components: a cross-agency enforcement team; the identification of individuals responsible for violence and their associated groups; the development of an enforcement strategy that targets all the individuals and their associates; direct messaging to the individuals that violence will no longer be tolerated and severe consequences will follow if the warning is not heeded; direct pleas from community members who also want the violence to end; and the offer of social services to support lifestyle and behavioral changes relevant to specific risk and protective factors (Kennedy, 2006; National Network for Safe Communities, 2013; Braga, Weisburd and Turchan, 2018). The threat of the law enforcement actions that will be taken against the identified individuals if the criminal activity does not stop is ideally communicated by those who receive the messages to other group-involved individuals to deter them from committing future acts of violence.

The focused deterrence program model was first developed and implemented in Boston under the leadership of criminologist David Kennedy in the 1990s; it has since been replicated in dozens of cities across the country. A 2018 systematic review found that 19 of 24 evaluations of focused deterrence programs were associated with strong, statistically significant crime reductions where implemented (Braga, Weisburd and Turchan, 2018). For example, Project Safe Neighborhoods, in Lowell, Massachusetts, was associated with a 44% reduction in gun assault incidents and no displacement effects of the intervention (Braga et al., 2008). The Group Violence Reduction Strategy in New Orleans, Louisiana, was credited with a 17% reduction in total homicides and firearm homicides, a 17% reduction in nonfatal firearm assaults, and a 32% reduction in group member-involved homicides (Corsaro and Engel, 2015). The similarly named Group Violence Reduction Strategy in Chicago, Illinois, was associated with a 32% reduction in shooting victimizations among groups represented at the call-ins, compared to those groups that did not directly receive the call-in messages (Papachristos and Kirk, 2015). Boston has implemented two versions of the group-member violence reduction strategy, once to address youth violence and again to curb a growing gang violence issue, and saw significant reductions in gun violence in both iterations of the program (Braga et al., 2001; Braga, Hureau, and Papachristos, 2014). The authors of the systematic review noted that the strongest crime reduction impacts have come from focused deterrence strategies concentrated on the most violent actors, and that focused deterrence approaches aimed at reducing crime associated with illicit drug markets had the smallest effects (Braga, Weisburd and Turchan, 2018). Additionally, several evaluation studies have found that threats to treatment fidelity can occur at several stages of program implementation, potentially undermining the success of the intervention in a given community (Corsaro and Brunson, 2013; Fox, Novak and Yaghoub, 2015; Saunders, Kilmer and Ober, 2015).

Focused Deterrence in Baltimore

Prior to Ceasefire in 2014, Baltimore had utilized pieces of the focused deterrence approach to supplement its violence reduction efforts, but due to leadership and strategy changes, the program was never fully implemented. In early 2014, however, Baltimore's mayor contracted with National Network for Safe Communities (NNSC) at City University of New York's John Jay College of Criminal Justice and began implementing the focused deterrence program (Fenton, 2014). The program leaders chose to focus the initiative on homicides and to begin implementation in the Western District of the city, due to the district's historically high rate of gun violence. Eighteen narcotics detectives and two sergeants from the Western District were chosen to be the Ceasefire unit within the Baltimore Police Department. The NNSC advisory team conducted an extensive problem analysis in conjunction with leaders and representatives from local, state, and federal law enforcement, correctional, and criminal justice agencies. The problem analysis included a case incident review of recent homicides in the Western District and a group audit to gather intel from front-line law enforcement personnel on the active and violent groups within the district. The advisory team also conducted a social network analysis, with the help of local law enforcement, to link violent individuals and groups to their associates.

The law enforcement arm of the Ceasefire intervention then held a meeting with heads of various city and state agencies to develop the enforcement strategy that would be executed if, following a call-in or custom notification, a member of the intended population was involved in a homicide. The Ceasefire program manager also invited service providers and leaders of community-based organizations to a meeting with both law enforcement and service providers to discuss the need and plan of action for supporting the individuals who would receive the call-in or notification messages.

Working through the Department of Public Safety and Correctional Services' Parole and Probation department, Operation Ceasefire Baltimore contacted approximately 40 individuals in the Western District believed to be involved in violent activity and directed them to attend the first call-in on June 10, 2014. All call-ins generally followed the program model, with city and law enforcement leaders first giving warnings of prosecution and crackdown if future shootings occur, and then members of the community who had experienced gun violence or undergone their own lifestyle changes away from violence sharing their stories and pleading for the violence to end. An offer of assistance was made to those individuals who wanted and needed help to put their lives on a path of nonviolence. Following the call-in, the enforcement strategy was put into action when a homicide that occurred anywhere in the city was tied to one of the groups represented at a prior call-in. The strategy directed all legal punitive actions against those involved and was carried out for approximately two to three months, at which time the next call-in occurred and the process repeated. To reach individuals who were linked to violence but were not on parole and probation and thus could not be directed by the Maryland Department of Public Safety and Correctional Services to attend a call-in, personal custom notifications were delivered to individuals' places of residence. The overall message of the custom notification was the same as that of the call-in but was conveyed on an individual basis versus in the group setting of the call-in.

A total of five call-in meetings was completed in the Western District. Additionally, the Ceasefire intervention expanded to the Eastern District in early 2015 and held four call-ins in the Eastern through 2016. The last call-in, in September 2016, was centrally located and included individuals from both Eastern and Western Districts. The dates for all call-ins are shown in Table 5.1. The Ceasefire program initially reported steep reductions in group-member-involved homicides and nonfatal shootings compared to the same time periods in the previous year (National Network for Safe Communities, 2018). However, following the in-custody death of Freddie Gray, Jr., and the civil unrest that primarily occurred in Baltimore's Western District in April 2015, the city experienced a sharp rise in homicides and nonfatal shootings, followed by a change in Baltimore Police Department leadership in July 2015 and a new priority violence reduction strategy within the police department announced in August 2015 (Campbell and Anderson, 2015). The Ceasefire intervention team continued to hold call-ins through September 2016 and deliver custom notifications through early 2017, but in June 2017, the program was declared to be no longer in operation (Rodricks, 2017).

Table 5.1: District and Dates for Operation Ceasefire Baltimore Call-ins

| **District** | **Date** |
|---|---|
| Western | June 10, 2014 |
| | September 24, 2014 |
| | February 5, 2015 |
| | July 15, 2015 |
| | November 12, 2015 |
| Eastern | March 31, 2015 |
| | August 27, 2015 |
| | February 24, 2016 |
| | July 16, 2016 |
| Western/Eastern | September 29, 2016 |

The overall aim of this study was to evaluate the impact of the Ceasefire program in Baltimore. The objective was to examine the spatial-temporal association between the Ceasefire call-ins and subsequent homicides or nonfatal shootings in the districts where the call-ins occurred and to gain insight into Ceasefire's structure, processes, and execution through supplementary qualitative data from key informants who managed and implemented the program.

## Methods

### Setting

The study was conducted in Baltimore, Maryland, a city that has had consistently high rates of gun violence. Baltimore has one of the highest violence rates per capita of American cities with populations greater than 250,000 (United States Department of Justice, 2015). The Ceasefire intervention was eventually implemented in two of Baltimore's nine police districts, Eastern and Western. The districts selected for Ceasefire are historically two of the most violent districts in the city; in the ten years preceding the intervention, 30% of all homicides and 31% of all nonfatal shootings in Baltimore occurred in either Eastern or Western (Baltimore Police Department, 2018).

### Study Design

A multiple interrupted time-series study design was used to examine the association between each Ceasefire call-in and gun violence within the districts where the call-ins occurred in the months following the call-ins, during which the Baltimore Police Department conducted enforcement on any groups whose members had attended the call-in or received a custom notification and were believed to be connected to a homicide after the law enforcement warning.

### Data Sources

The outcomes of interest for this study were homicide or nonfatal shooting counts, as they are the most reliably tracked incidents of violence and the majority of homicides committed in Baltimore involve firearms; over 84% of all Baltimoreans killed in the past six years died by gun violence (Baltimore Police Department, 2018). Additionally, although the Ceasefire intervention was officially focused only on homicides, several key informants suggested that the intervention was sometimes applied to individuals involved in nonfatal shootings as well. The Baltimore Police Department (BPD) directly provided individual-level information on all homicides and nonfatal shootings between January 1, 2003, and November 30, 2015. The homicide and nonfatal shooting data from BPD included the date and physical location of the incident, as well as the victim's date of birth, gender, and age. Data for homicides and nonfatal shootings occurring between December 1, 2015, and December 31, 2017, were obtained through the city of Baltimore's Open Data catalog for BPD Part I Victim Based Crime Data (Open Baltimore, 2018). These data are updated weekly by BPD and, in addition to the incident date, include the street block, weapon type, premise, and geographic coordinates of each reported crime incident, but they do not include any victim-specific information.

To assess whether the Ceasefire program effects are mediated by increases in drug or gun law enforcement, drug possession, drug trafficking/distribution arrests, and illegal weapon possession arrests were included as independent variables. Data on individual-level arrests for drug possession, drug trafficking or distribution, or illegal weapon possession violations were obtained directly from BPD for the years 2003-2015 and from Baltimore's Open Data catalog for BPD Arrests (Open Baltimore, 2018) for years 2016-2017.

The dates and districts for the Operation Ceasefire Baltimore call-ins were obtained from the Baltimore Mayor's Office on Criminal Justice, which staffed the Ceasefire program manager position and was tasked with oversight of the intervention.

## Measures

The dependent variables for this study were homicide or nonfatal shootings. The explanatory variables were drug busts, drug possession arrests, drug trafficking/distribution arrests, and weapon possession arrests, as well as a variable indicating the months following the civil unrest. The homicide, nonfatal shooting, and arrest locations were geo-located as points and then aggregated to the police post polygon level using a shapefile of BPD's police posts (142 police posts in Baltimore). The data were then totaled for each post for each month between January 2003 and December 2017 (n=180 months per post). The arrest variables were lagged by one month (t-1) to address endogeneity concerns (e.g., shootings can spur increased enforcement in a given area and time). Each police post was assigned to its corresponding district number so that the post-level data could also be aggregated to the police district level. An indicator variable was created to denote the months following the civil unrest in late April 2015, with "0" representing the months prior to the unrest and "1" representing the months following the unrest.

Although the geographic territories or primary locations of individuals and groups identified as being the most violent in the Eastern and Western Districts were mapped to neighborhood blocks for the purpose of the Ceasefire problem analysis, the individuals and groups themselves were obviously not physically constrained, so enforcement tactics, as well as the messages of deterrence and assistance, were also not tightly restricted to just a neighborhood or a police post. Thus, one independent variable, representing the time period following a Ceasefire call-in, was coded as an indicator variable, with "1" representing an after-call-in month in a police post in the Western or Eastern District and "0" otherwise. If a call-in occurred after the $15^{th}$ of the month, the indicator variable was turned on beginning the following month. An additional independent variable was created to indicate the cumulative total of the call-ins that had occurred at a given time, so that each subsequent call-in contributed to the additive effect of the intervention in the Western or Eastern District. The independent variables were coded such that there were two variables for each district.

Analytic Strategy

A police post-month panel dataset was created and regressions were run using generalized linear models with a negative binomial distribution. Robust standard errors were specified to account for intragroup correlation that may occur by police post. A Durbin-Wu-Hausman test was conducted to confirm the use of fixed versus random effects due to correlation between the unobserved effects and the explanatory variables. Dummy variables for month and year were included in the models to control for seasonality and other unmeasured time-variant changes in factors. To control for post-specific trends throughout the study period that could contribute to variances in homicides or nonfatal shootings by post, a dummy variable for post was also included. Estimates were exponentiated and are presented as incident rate ratios with associated p-values; asterisks delineate the estimates that were significant at the $p<0.05$, $p<0.01$, and $p<0.001$. All geocoding of point data and aggregation to police post polygons was completed using ESRI Business Analyst 2015 software in ArcGIS Desktop 10.4.1 (ESRI, 2015). All negative binomial regressions were performed in Stata/IC 15.1 for Mac (64-bit Intel) (StataCorp, 2017). Data management was shared between the two software platforms.

The quantitative analysis was supplemented with semi-structured, one-on-one interviews with six key personnel who were instrumentally involved in the Ceasefire intervention. A copy of the interview guide is included in Appendix C. The key informants were employees of the Baltimore Police Department, Baltimore State's Attorney's Office, or the Mayor's Office during the design, implementation, and/or execution of Ceasefire. Recruitment of the key informants was conducted based on the researcher's knowledge of the Ceasefire intervention through the researcher's employment in the Baltimore City Mayor's Office from February 2013 through November 2015. The key informants were initially contacted for the study by cell phone text, email, or Facebook Messenger. Once the informants agreed to talk by phone, they were called, provided an explanation of the study, asked to participate. All six of the individuals contacted agreed to be interviewed. The interviews were conducted face-to-face in restaurants or office spaces. The interviewees were instructed that their responses were confidential and would not be reported in a manner that could lead to their identification. The interviews ranged from 30 to 75 minutes and were recorded on a laptop using QuickTime Player audio recording software. Notes were also taken during the interviews. After each interview, the recordings were replayed and the notes were edited to ensure the details of each conversation were documented in the notes. A grounded analysis was used to identify themes as they emerged from the interview data. The notes from the first interview were reviewed and organized into themes, which were used to create a codebook. The notes from each subsequent interview were reviewed, and words, phrases, or sentences were then organized using the codebook. New themes that emerged were added to the codebook. After reviewing all notes once, the notes were reviewed a second time to ensure that content from each interview was properly organized into the appropriate thematic category.

This study was deemed as "not human subject research" by the Johns Hopkins Bloomberg School of Public Health.

## Results

For the study period of January 2003 through December 2017, there were 561 homicides and 1,278 nonfatal shootings geo-located to the Western District of Baltimore; there were 592 homicides and 1,334 nonfatal shootings geo-located to the Eastern District. Three-month moving averages for these outcomes are presented in Figures 5.1 and 5.2. The graphs also illustrate the month of the first Ceasefire call-in in the district. Baltimore City as a whole experienced a sharp spike in homicides and nonfatal shootings following the civil unrest in late April 2015.

Figure 5.1: Three-Month Moving Averages for Homicides and Nonfatal Shootings in Western District, 2003-2017

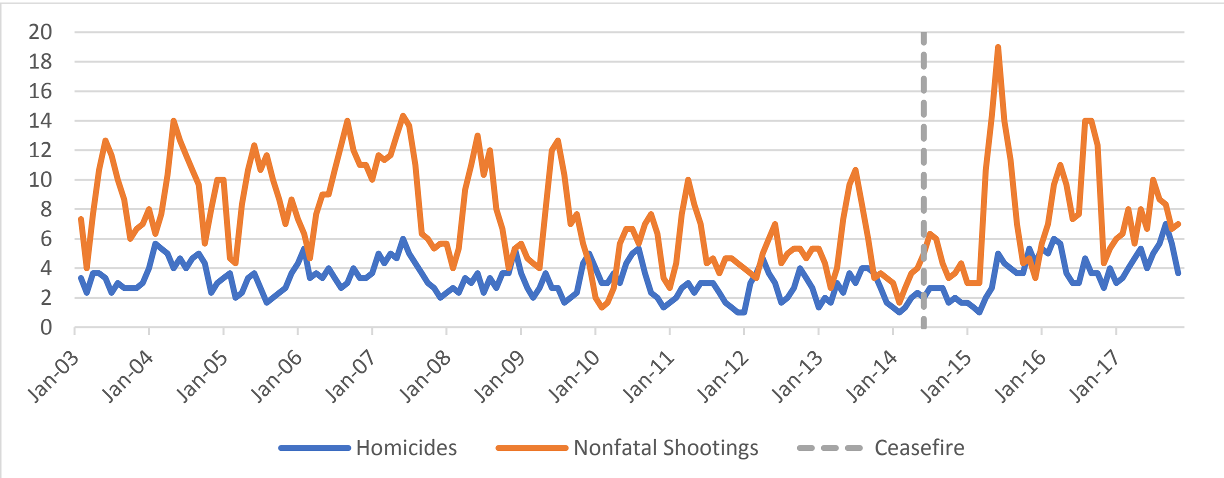
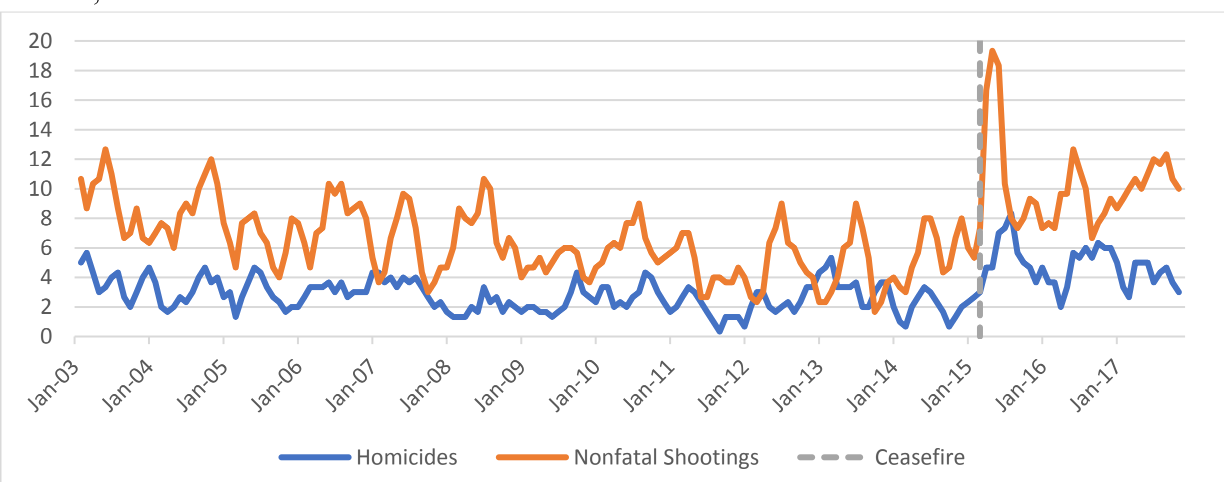



Figure 5.2: Three-Month Moving Averages for Homicides and Nonfatal Shootings in Eastern District, 2003-2017




The regression analysis was first run without the arrest control variables and then with the variables included to detect any mediation effect from the arrests. The estimates from both analyses are presented in Table 5.2. The presence of the Ceasefire intervention did not have an appreciable impact on homicides or nonfatal shootings. However, the cumulative effect of the intervention was positively associated with homicides, meaning that, with every call-in, homicides increased by 8% (IRR: 1.080, p=0.020). The overall cumulative intervention effect was driven by the Western District, which had an 11% increase in homicides with each subsequent call-in. The presence of the intervention in the Western District was associated with a 36% increase in nonfatal shootings (IRR: 1.360, p=0007). The impact of the intervention, whether in general or when considering the cumulative effect of the call-ins, was not mediated by drug- or weapon-related arrests.

Table 5.2: Results from Negative Binomial Regressions for Homicides or Nonfatal Shootings with Treatment Effect of Intervention, IRRs

| | Main Model | | | | Main Model + Arrest Variables | | | |
|---|---|---|---|---|---|---|---|---|
| | Homicides | | Nonfatal Shootings | | Homicides | | Nonfatal Shootings | |
| Any Ceasefire | 0.843 | (0.233) | 1.088 | (0.425) | 0.837 | (0.215) | 1.087 | (0.429) |
| Cumulative Ceasefire | 1.080* | (0.020) | 1.003 | (0.928) | 1.081* | (0.019) | 1.003 | (0.927) |
| Ceasefire-Western | 0.796 | (0.266) | 1.360** | (0.007) | 0.793 | (0.259) | 1.359** | (0.007) |
| Cumulative Ceasefire-Western | 1.113* | (0.015) | 0.979 | (0.531) | 1.113* | (0.016) | 0.979 | (0.531) |
| Ceasefire-Eastern | 0.954 | (0.774) | 0.958 | (0.746) | 0.944 | (0.728) | 0.952 | (0.719) |
| Cumulative Ceasefire-Eastern | 1.002 | (0.971) | 0.979 | (0.615) | 1.002 | (0.969) | 0.978 | (0.596) |
| Drug Possession Arrests | | | | | 0.999 | (0.804) | 1.003 | (0.257) |
| Drug Trafficking Arrests | | | | | 0.998 | (0.690) | 0.997 | (0.518) |
| Weapon Possession Arrests | | | | | 0.999 | (0.935) | 0.995 | (0.664) |
| Post-Unrest | 1.494* | (0.011) | 1.723*** | (<0.001) | 1.494* | (0.011) | 1.733*** | (<0.001) |

Exponentiated coefficients; p-values in parentheses
* p<0.05, ** p<0.01, *** p<0.001

The key informant interviews offered important insights about successes, challenges, and concerns with Operation Ceasefire Baltimore's implementation and execution. The responses from the interview are presented based on themes that emerged from the discussions.

Intervention Partnerships and Concept Buy-In

Each key informant talked about the initial collective support and buy-in from all partners of Ceasefire as being one of the intervention's greatest successes. As one interviewee said, "this was the second go-round in Baltimore, so people really wanted to see it succeed and worked to make it happen." There was a high level of commitment from all relevant agencies: The Mayor's Office, the Baltimore Police Department, the State's Attorney's Office, Parole and Probation, the United States' Attorney's Office, the Department of Public Safety and Correctional Services, the High Intensity Drug Trafficking Area office in the Maryland/Virginia/DC region, Code Enforcement, Department of Transportation, Department of Social Services, Animal Control, and more. All of the representatives from those agencies who came to the table held the appropriate positions to demonstrate the importance of the program and to collaboratively develop and execute strategies and action plans for the intervention. There was also cautious but optimistic buy-in from the social service providers and community-based organizations that were requested to extend support to the individuals who took up the offer of assistance and help to change their lives. Although several of the community leaders expressed apprehension about trusting law enforcement to correctly identify the violent individuals and groups and to work collaboratively with community members to implement the program with precision and diplomacy, as one interviewee put it, "it originally united the police and the Mayor's Office and some community leaders around this idea that violence needs to stop and we want to move to a place where communities are safer."

Identification of Violent Individuals and Groups

The interviewees were in agreement with their articulation of the overall objectives and goals of the Ceasefire intervention. The focus of the program was on homicides committed by group-involved individuals, and the program would identify individuals linked to groups that had participated in violence, send the message to those individuals and groups that they would face severe sanctions and charges for any and all law violations if they committed a future act of violence, and then follow through with intense enforcement to demonstrate that the warning was legitimate, using a "root to branch" approach of cracking down on everyone in the group.

Several of the interviewees expressed concerns about the accuracy of the problem analysis and the individuals identified as the intended audience for the intervention. The qualitative component of the problem analysis relied heavily on the perceptions of front-line law enforcement personnel, which, to some, was arbitrary and anecdotal instead of being data-driven. There were also questions about the reliance on Parole and Probation to reach individuals for the call-ins. If individuals were not on parole or probation, there was no way to make them attend a call-in, and many of the individuals involved in violence were not on parole or probation, so the ability to reach a number of violent actors was limited. Some interviewees did suggest that the one-on-one nature of the custom notifications allowed for a more personal and directed conversation to the individual and/or his or her family members who were present. Overall, though, there was skepticism among the interviewees that the right individuals – those committing violence in the districts - were not consistently identified and reached by the intervention, as homicides that were not connected to any of the groups in the Ceasefire database continued to take place.

Call-ins and Enforcement

Nearly every Ceasefire call-in was attended and involved an address by the mayor, the police commissioner, the State's Attorney or a representative from the office, a representative from the Assistant United States' Attorney's Office, and the major in the corresponding police district. There were also presentations from at least one formerly incarcerated individual and a mother of a gun violence victim or the member of the clergy who pleaded with the call-in attendees to stop the violence. The Ceasefire program manager spoke as well, explaining the service offerings that would be available if the individuals decided that they wanted to begin the process of changing their lives.

The key informants had mixed perspectives on the effectiveness of the call-ins to appropriately deliver the message that violence would not be tolerated. Some felt that the delivery of the warnings regarding future violence was clear and meaningful. Others objected to the manner in which the warnings were delivered and felt that the leaders who threatened harsh consequences were unable to skillfully connect with the individuals and authentically convey the "stop the violence" intent of the intervention. More than one interviewee expressed concern that the message at the call-ins was supposed to focus solely on ending the violence, but that often the message that was instead conveyed to the call-in attendees was that they needed to "get out of the (drug) game" or else they would face the heavy hand of the law.

Following the call-ins, the enforcement strategy, as previously described, was employed if any future homicides were linked to individuals or groups represented at the prior call-ins. The interviewees overall expressed satisfaction with the initial enforcement strategy and felt that the intervention led to reductions in group-involved homicides and the arrests of key violent individuals, particularly in the Western District. Some said that the focus on extracting entire groups of violent actors was far more successful than arresting a few individuals at a time and leaving the opportunity open for lower-level group members to step up and simply continue the group's activity. Others talked about the high level of coordination among the Ceasefire partners to execute the enforcement strategy and use "every kind of law enforcement possible" to crack down on the violent individuals and their associates, including outstanding warrants, drug or weapon possession, illegal cable or electricity connections, traffic tickets, or any other law violations, and build conspiracy, kingpin, or Racketeer Influenced and Corrupt Organizations Act (RICO) cases against them. Weekly intel meetings with all law enforcement and agency partners allowed for feedback sharing on the strategy. The intervention appeared to be spreading by word of mouth; at least one incarcerated individual was recorded on a call saying, "they're coming after everybody." However, there were again concerns among some interviewees about the extent to which individuals had been accurately linked by the Ceasefire team to groups involved in violence and how they were surveilled by Ceasefire detectives. One interviewee recalled multiple occasions when individuals stated at the call-ins or afterward to a member of the Ceasefire team that they were not involved in violence and were upset by the perceived association. Several interviewees also discussed uneasiness about the lack of data produced about who was getting arrested and for what.

Social Service Support and Community Engagement

The key informant interviewees universally agreed that the greatest weakness of the Ceasefire intervention was the lack of genuine social service support offered to individuals who wanted to stop being involved in violence. The budget for Operation Ceasefire Baltimore only covered technical assistance to the National Network for Safe Communities advisory team and the Ceasefire program manager. City officials and Ceasefire program leaders disagreed publicly about the need for additional upfront resources for social services (Fenton, Broadwater and Donovan, 2015), but nearly all of the interviewees in this study expressed concern that there were no wraparound rehabilitation services offered to these individuals prior to their targeting by law enforcement, and that the services provided through Ceasefire were insufficient. There were no funds provided to the service providers who were asked to partner with the intervention and to prioritize assistance to this highest-risk and extremely disconnected population. The city's Community Action Partnership, which is operated by the Mayor's Office of Human Services and is devoted to supporting Baltimore's low-income population, was found to be ill-equipped to handle the need, lacking capacity and resources. And while some interviewees cited a low rate of individuals reaching out at or after the call-ins to accept help from the city, others pointed to the city's inability to provide adequate support to the few individuals who did seek assistance and the impact that the failure to deliver on the promise of assistance would have on others. As one interviewee stated, "[Robust support services and outreach] has to have the same legitimacy as the enforcement side. Otherwise, you're selling a bill of goods." Another interviewee was more pointed: "a carrot and stick model with only the stick is basically contributing to mass incarceration."

Another related expressed concern of the Ceasefire intervention was the lack of engagement with the community about the intervention, its objectives, and the overall perception and success of the program. According to several interviewees, crucial opportunities were missed to actively engage the Ceasefire communities in candid conversations about reconciliation, the importance of demonstrating fairness and respect, and ways in which they might work together to achieve a shared goal of, and singular focus on, making their neighborhoods safe from violent crime.

Other Program Shortcomings

All interviewees talked about the unrealized potential of the Ceasefire intervention and each pointed to certain steps or decisions that significantly altered the course of the program. In addition to the lack of resources available for social service support and outreach, the police department was given no additional resources to operate the Ceasefire unit, which was comprised of two sergeants and eighteen detectives. Thus, after approximately six months, the Ceasefire unit, which had originally been a supplemental crime-fighting strategy in the Western District, was left to be the primary strategy in the Western District due to resource constraints. The intervention then expanded to the Eastern District in early 2015, further taxing the department by pulling experienced detectives from other operations and specialized units in the district without replacement. There was also no dedicated data analyst within the police department for long stretches at a time, which raised concerns about the department's ability to continuously analyze data to ensure that the individuals sought through the intervention were the ones responsible for violent activity. As one interviewee said when speaking to the shortage of resources available to BPD for this new intervention, "police with fewer resources aren't going to be better police." Although there was a sense of urgency among some in leadership to move quickly to expand the intervention to the Eastern District and to make Ceasefire the primary violence reduction approach in the city, all interviewees talked about the expansions as examples of the lack of patience among Baltimore's city leaders to "give a good thing time to get better." Another interviewee stated, "Baltimore's problems are long-term problems. The city is looking for a quick fix, but instant gratification isn't gonna be won here."

The interviewees also spoke to the lack of continuity in leadership as a major challenge to Ceasefire's success. Within approximately eighteen months of the first call-in, there was the election of a new State's Attorney, the resignation of the deputy mayor who oversaw the Baltimore Police Department and the Mayor's Office on Criminal Justice, as well as the resignations of the original Ceasefire program director, the resignations of the director and assistant directors of the Mayor's Office on Criminal Justice, the termination of the police commissioner, the reassignment of the BPD Ceasefire program coordinator, and the reassignment of one of the original leaders of the BPD Ceasefire team. Although the NNSC advisory team continued to support the Ceasefire intervention throughout the leadership changes, and Ceasefire continued to conduct call-ins through September 2016, the interviewees talked about the difficulty of maintaining organization and keeping external partners at the table. Several interviewees also spoke to a shift in priority in BPD after the appointment of a new police commissioner in July 2015. Facing historic increases in homicides and nonfatal shootings, BPD announced a new violence reduction strategy that concentrated on violent individuals versus groups, and the enforcement strategy reportedly shifted accordingly, relying increasingly on custom notifications to reach individuals identified as violent. Additionally, the weekly intel meetings ceased, and though BPD maintained a Ceasefire unit in both the Western and Eastern districts, the level of collaboration and coordination among the various agencies diminished, and the BPD program coordinator was replaced with someone who did not have rank over the detectives within the unit, leading to less strategic organization.

Finally, all interviewees recognized the incredible legitimacy challenge faced by police after the civil unrest in April 2015. The in-custody death of Freddie Gray on April 19, 2015, helped to intensely ratchet up tensions between police and Baltimore city residents, and the frustration and anger boiled over on April 27, 2015, resulting in massive protests, looting, and intense encounters between law enforcement and community members. After the subsequent weeklong citywide curfew was lifted, police were confronted with new challenges when conducting day-to-day operations and investigations, with routine stops drawing crowds of residents and some confrontations leading to police confronting crowds while in riot gear and being pelted with rocks. These dynamics virtually erased the legitimacy of the police department in the eyes of many community members; not only was the threat of arrest "no longer present," as one interviewee stated, but new questions emerged about the department's ability to perform procedurally and justly.

## Discussion

Operation Ceasefire Baltimore was implemented in 2014 with the intention of bringing an evidence-based strategy with proven success in other cities to Baltimore at a time when homicides and nonfatal shootings, while generally on the decline over the previous years, remained stubbornly high in the two most violent districts in the city. This study found that the program effect was in the harmful direction for homicides and nonfatal shootings in the Western District. Each call-in in the Western District was associated with an 8% increase in homicides. Furthermore, the presence of the intervention in the Western District was associated with a 36% increase in nonfatal shootings. Ceasefire personnel reported initial reductions in group-involved homicides and nonfatal shootings in both the Western and Eastern Districts, but the analyses in this study did not yield similar reductions. The discrepancy between the initial internal program findings and the findings in this study could possibly be explained by different criteria used to assess program success. This research did not examine individual- or group-level violence because of the police department's restrictions on sharing personally identifiable data and law enforcement intel with external researchers.

The key informant interviews shed light on the promise and initial qualitative successes of the intervention but also revealed how various elements of the focused deterrence program model were not properly implemented in Baltimore. Per the interviewees, the lack of funding for the intervention greatly compromised its ability to properly equip the police department and social service providers with the data and resources necessary to both consistently identify the individuals most responsible for violence and to deliver on services needed to support those who wanted them. Furthermore, the interviewees believed that the frequent turnover in key leadership positions jeopardized the consistency in messaging to internal enforcement units and external partners.

Having an ill-framed message at the call-ins, with law enforcement possibly telling individuals that they must "get out of the game" or face penalties versus focusing entirely on gun violence, is an important concern. Individuals have various motivations for joining gangs or groups (Taylor, 2013; Sanchez-Jankowski, 1991; Grant and Feimer, 2007; Klemp-North, 2007), and the sense of structure, belonging, and loyalty related to group membership, or even concerns about consequences of disavowing their group, may cause individuals to dismiss the message of the call-in entirely. Similarly, factors like market forces that impact profits to be made from selling illicit drugs, or the lack of economic opportunities for many individuals who sell illicit drugs could lead those who receive the call-in message as a demand for them to stop selling illicit drugs to discount the legitimacy of the intervention as a viable alternative to their lifestyles. Indeed, most evaluated focused deterrence strategies that have directed their efforts towards illicit drug market-related violence have not been found to be effective at reducing gun violence (Braga, Weisburd and Turchan, 2018).

The importance of proactively addressing community distrust of law enforcement and gaining the community's trust and acceptance of the enforcement strategy cannot be understated. Residents in Baltimore have long reported high levels of distrust of law enforcement (Leger, 2015; The Melior Group, 2015), and as a 2016 United States Department of Justice Civil Rights Division report stated, the Baltimore Police Department has consistently "engaged in a pattern or practice of conduct that violates the United States Constitution and laws, and conduct that raises serious concerns" (United States Department of Justice, 2018). Simultaneously, the Baltimore Police Department has a distressingly low rate of homicide arrests, with nearly 60% and 70% of all murders occurring without arrests in 2014 and 2015, respectively (Maryland Department of State Police, 2016). The inverse relationship between community trust and homicide clearance rates has been documented in prior research (Wellford and Cronin, 2000).

Research has also shown that incidents of police misconduct have a depressing effect on the community's willingness to cooperate with law enforcement and to engage the police when violence occurs (Desmond, Papachristos and Kirk, 2016). Zero-tolerance policing in Baltimore during the early 2000s, applied disproportionately to black males, led to the arrests of thousands of residents for low-level offenses and greatly damaged the relationship between the community and the police (Collins, 2007; The Real News, 2016). The Ceasefire enforcement strategy relied on the use of all available legal levers to arrest individuals and their associates when homicides occurred, but without effective communication with the community about the strategy and its goals, the arrests may have appeared to be yet another tool for sweeping apprehensions of black men. The potential failure of the police department to connect with community members about the intervention's objectives and to openly discuss and adequately address valid community concerns would likely have had significant consequences for the program's ability to identify those individuals most responsible for gun violence in the districts. Focused deterrence relies on the perceived legitimacy of law enforcement in the eyes of the violent individuals and groups, as well as residents, community-based organizations and service providers, to be successful. As stated in the systematic review of program evaluations: "in the focused deterrence approach, emphasis is not only on increasing the risks associated with offending, but it is also on decreasing opportunity structures for crime, deflecting offenders away from crime, increasing the collective efficacy of communities, and increasing the legitimacy of police actions." (Braga, Weisburd and Turchan, 2018, p. 8).

Future research should integrate the viewpoints of a member of the NNSC advisory team to better understand the achievements and challenges the team encountered when attempting to advise city leadership on the implementation of a strategy that requires such attention to detail, appropriate leadership, and community engagement. The focused deterrence model, as earlier stated, has been largely found to have violence-reducing effects in the areas where it has been fully implemented. It is possible that the advisory team can provide greater awareness of additional implementation and sustainability challenges encountered in Baltimore that were overcome in other cities. Future research should also seek to identify the specific components of the focused deterrence model that are most critical to the success of the intervention.

Important limitations of this research must be noted. First, as mentioned, access to the Ceasefire database of the names of identified violent individuals, their groups, or their geographic locations was not granted, so the researcher was unable to more precisely measure enforcement that may have disrupted or reduced violence at a more granular level than police district. Also, without details about specific enforcement activities taken against individuals, there could not be an examination of whether certain enforcement tactics had more protective or harmful effects on subsequent gun violence than others. Moreover, while key Ceasefire personnel about the design, implementation, and execution of the program were interviewed for this research, service providers, who were tasked with engaging and supporting individuals who sought assistance and who could potentially offer different perspectives of the program's successes and challenges, were not contacted for interview. There was additionally the potential for recall bias by the interviewees, given the time lapse between their involvement in the intervention and the interviews for this study. However, the general consistency in the responses across participants suggests that concerns regarding this internal validity threat were minimized.

This study is the first to consider systems-level processes and actions related to the effectiveness and execution of Operation Ceasefire Baltimore. It incorporated invaluable insight from key local players in the intervention, who provided important assessments of how the program fit within the larger context of the socio-political environment in Baltimore at the time. It also offers vital feedback to city leaders in Baltimore and beyond who are considering the implementation of a focused deterrence program as a violence reduction strategy.

## Discussion

### Findings

The first study in this dissertation research project sought to understand the relationship between drug law enforcement and gun violence by considering how small- and large-scale arrests of individuals involved in the illicit drug trade may lead to changes in homicide or nonfatal shooting rates in the areas where the arrests or drug activity occurred. The analyses suggested that drug possession arrests, drug trafficking arrests, and major drug busts, regardless of the presence of federal agency involvement or the potential apprehension of violent individuals in those busts, did not led to an appreciable reduction in homicides in Baltimore over the fifteen-year study period. Major drug busts overall also did not lead to reductions in nonfatal shootings. There was evidence that major drug busts of individuals who were allegedly and explicitly linked to violence may have some protective effect on nonfatal shootings, but this needs to be further explored with future research, given that most estimates were not significant and this analysis only found protective effects when comparing those busts to busts of individuals not explicitly linked to violence. Drug possession arrests, as well as drug trafficking/distribution arrest surges, were associated with statistically significant increases in nonfatal shootings in some of the months following their occurrences. There was also an indication that drug possession arrests could lead to displacement of illicit drug activity, though further examination of the dynamics of illicit drug markets following arrests is needed.

The second study in this research project sought to closely examine the effects of Safe Streets using a statistical approach that allows for a comparison of observed homicide and nonfatal shooting rates after the program's implementation to the rates we would have expected if the program had not been implemented. The synthetic control analyses found that only one Safe

Streets site – McElderry Park - had reductions in homicides that approached significance over the course of the study period.  Two sites that have been shut down, Mondawmin and Madison-Eastend, experienced dramatic increases in both homicides and nonfatal shootings after the sites opened, with Mondawmin's largest increases occurring after the unrest.   An examination of program effect attenuation over time revealed uneven effects as well.

The third study in this dissertation research examined the effect of Operation Ceasefire Baltimore on reducing gun violence in the Western and Eastern Districts.  The study found that the program effect was in the harmful direction for homicides and nonfatal shootings in the Western District.  Each call-in in the Western District was associated with an 8% increase in homicides.  Furthermore, the presence of the intervention in the Western District was associated with a 36% increase in nonfatal shootings.  Key informant interviews shed light on the promise and initial qualitative successes of Ceasefire but also revealed how various elements of the focused deterrence program model were not properly implemented in Baltimore.  Per the interviewees, the lack of funding for the intervention greatly compromised its ability to properly equip the police department and social service providers with the data and resources necessary to both consistently identify the individuals most responsible for violence and to deliver on services needed to support those who wanted them.  The interviewees believed that frequent turnover in key leadership positions jeopardized the consistency in messaging to internal enforcement units and external partners.  Also, questions were raised among the interviewees about program's ability to succeed in Baltimore without active engagement with community members and actions taken to increase the Baltimore Police Department's legitimacy in the eyes of residents.

### Limitations

The limitations of this research are important to acknowledge. First, this project relied solely on data related to programs implemented in Baltimore, Maryland. Thus, the generalizability of the findings may be limited, although the results were not incongruent with findings from previous evaluations of these programs in other jurisdictions. Also, the use of police arrest data as a measure of the extent and location of illicit drug markets is restrictive in that it reflects the actions of the police department itself, rather than capturing an accurate measure of drug market-related activity. Lagged effects of the drug law enforcement interventions and arrests for drug and weapon law violations were included in this study to help minimize concerns of endogeneity, but the challenge remained in being able to account for drug market locations and shifts in those markets that may be associated with law enforcement activity. This study also was unable to incorporate federal drug-related arrest data, which are not publicly available but would help provide a fuller picture of all law enforcement-related activity to disrupt drug markets. Another limitation of this project is that there were necessary assumptions made about the appropriate geographic unit of analysis. Gun violence is more likely to occur at the street or block level, and thus the aggregation of data to the police post level can result in loss of precision. However, prior research on gun and drug market-related violence has utilized police beats or police posts as the unit of analysis, primarily since this is the smallest unit for which law enforcement-specific incident-level data are available, and effect sizes have been detected at this unit in numerous studies, including this one.

One limitation related to the use of the synthetic control method for the Safe Streets evaluation is that the amount of observation data available to estimate either the pre-intervention outcome trends or the post-intervention program effects varied by site. The synthetic control models for Safe Streets in Mondawmin and Sandtown, which began in 2013 and 2016, respectively, had better model fit, per their RMSPEs, than did sites such as McElderry Park and Cherry Hill. Conversely, the earlier sites benefited from much more post-intervention data than did the sites that opened more recently. There was no appealing strategy for addressing this limitation. However, none of the RMPEs were found to be large, and the examination of program effects over different time periods, although the findings were not significant, provided insight into variance across sites over time, irrespective of program length. Another limitation is that the synthetic control model is unable to account for breaks in the intervention, such as the program suspensions in McElderry Park and Mondawmin. The suspensions were of short duration and thus likely did not have a major impact on the program's overall effect, but the breaks could nonetheless be incorporated into the models. Similarly, the program effects following the civil unrest in April 2015, which was a known shock to many neighborhoods in Baltimore, could not be isolated.

Access to the Ceasefire database of the names of identified violent individuals, their groups, or their geographic locations was not granted for this dissertation project, so the researcher was unable to more precisely measure enforcement that may have disrupted or reduced violence at a more granular level than police district. Additionally, without details about specific enforcement activities taken against individuals, there could not be an examination of whether certain enforcement tactics had more protective or harmful effects on subsequent gun violence than others. Another limitation in the Ceasefire evaluation was the possibility of recall bias by the key informants interviewed for the project, given the amount of time that passed between their involvement in the intervention and the interviews. However, the general consistency in the interviewees' responses suggested minimal likelihood of recall bias threatening the internal validity of the study.

**Policy Implications**

The findings from this dissertation research project were largely congruent with existing literature on the relationship between drug law enforcement and gun violence in cities in the United States and countries around the world. These results suggest that drug law enforcement has little, if any, protective impact on gun violence, and is more likely to instead be associated with increased, versus decreased, violence in the neighborhoods where the interventions occur. This research contributes to the literature by providing a 15-year analysis of the impact of drug law enforcement activity on gun violence in a city with high rates of both illicit drug activity and gun violence. It also showed that the negative effects of drug law enforcement may take months to realize. Policymakers at the local, state, and federal levels must seriously consider the harms of drug control policy versus its benefits. By continuing to invest heavily in drug law enforcement strategies that are not increasing public safety, they are undercutting efforts and investments that could provide the public with effective and sustainable public safety strategies. There must be candid discussions about the true costs of current drug law enforcement policies and the implementation of new strategies that will improve the quality of life for all citizens and communities impacted by illicit drug markets without imposing harm to communities, especially those that are already plagued with disparate rates of community violence.

The findings in the analysis of the Safe Streets program's effects on gun violence were also not incongruent with existing literature, which suggests that replications of the Cure Violence model have yielded inconsistent program-related reductions in homicides and nonfatal shootings.

Recent evaluations of model implementations have found encouraging evidence of the program's ability to effectively reduce gun violence, but taken in aggregate, evaluations of the program model have shown that the protective effects have differed across space and time, as seen in this study. Successful replications of the Cure Violence program in New York City and Philadelphia have been supported by both the mayor's offices and foundations in those cities. Importantly, these programs have been strengthened with financial resources for their staff and clients, as well as wraparound services for the individuals engaged by outreach workers. Future research in this area should closely examine the components of successful replications of the Cure Violence model to better understand how factors such as worker salaries, number and type of services available to program participants, collaboration with community-based organizations, and program oversight may explain discrepancies in program impact. Additionally, it is possible that differences within neighborhood-level behaviors may affect how outreach work and conflict mediation take place. For example, a cross-sectional study of conflict mediation records in Baltimore found that program-associated reductions in homicides were associated with a higher proportion of gang-related conflict mediations, while neighborhoods without similar program-associated homicide reductions saw more weapons and retaliatory conflicts (Whitehill, Webster and Vernick, 2013). Thus, additional analysis of norms and behaviors within the neighborhoods where Safe Streets is or will operate may allow for a more appropriately tailored approach to conflict mediation and violence interruption.

The Ceasefire evaluation offers important insights into challenges regarding the implementation and execution of focused deterrence programs. In addition to legitimate questions about leadership stability and program funding raised during key informant interviews, the concerns expressed about the identification of the right individuals most responsible for gun violence underscore the importance of utilizing accurate data and information to inform law enforcement practices aimed at increasing public safety. Furthermore, the importance of proactively addressing community distrust of law enforcement and gaining the community's trust and acceptance of the enforcement strategy cannot be overstated. Residents in Baltimore have long reported high levels of distrust of law enforcement (Leger, 2015; The Melior Group, 2015), and as a 2016 United States Department of Justice Civil Rights Division report stated, the Baltimore Police Department has consistently "engaged in a pattern or practice of conduct that violates the United States Constitution and laws, and conduct that raises serious concerns" (United States Department of Justice, 2018). Research has found that the perceived legitimacy of law enforcement is essential to lowering crime rates and eliciting cooperation from communities to help make them safer (Tyler and Fagan, 2008), and that compromised police legitimacy is a predictor of violent crime in structurally disadvantaged neighborhoods (Kane, 2005) such as those that received the Ceasefire intervention. Addressing the contentious relationship in Baltimore between law enforcement and community residents is critical to the success of the police to lower gun violence and increase the public's trust.

## Conclusion

The benefits of a fuller understanding of the relationship between drug law enforcement and related violent crime as it relates to public health policy are quite noteworthy. The impact of drug law enforcement and related sentencing policies on the increase in incarceration in America over the past forty years has been well documented. The number of individuals incarcerated in United States jails and prisons for drug law violations has grown from approximately 41,000 in 1980 to over 488,000 in 2014 (The Sentencing Project, 2016). However, rates of overall drug use, based on findings from large-scale population surveys, have remained fairly constant in the United States since the 1990s (Schulden, Thomas and Compton, 2009), suggesting that the increases that we have seen in arrests and incarceration for drug use and sales have not translated into substantial gains related to the goal of drug policy to decrease supply and thus decrease demand for drugs. Furthermore, the findings in this study are in alignment with the growing evidence that drug law enforcement does little to improve public safety and instead can lead to violence increases.

We are currently experiencing a national debate around the role and tactics of American policing and the benefits and harms of traditional approaches. While many of the individuals in the communities impacted by illicit drug markets have experienced the devastation of illicit drugs themselves or through their friends' or family members' experiences with drugs, there is increasing awareness that drug laws are not being enforced evenly, with individuals and communities of color being disproportionately affected by punitive approaches to drug laws despite no evidence that substance use, misuse, and/or dependence rates are significantly different among black and white populations (Schulden, 2009; Swendsen et al., 2012; Delker, Brown and Hasin, 2015). This disparate impact of drug law enforcement not only harms the communities that police are sworn to protect, but it also helps to drive resentment and distrust among communities and police. Providing a clearer picture of this association between interventions and outcomes to law enforcement agencies, local governments, and communities can allow for more efficient and effective strategy development and resource allocation aimed at violence reduction so as to minimize unintended consequences and enhance public safety.

The Safe Streets program in Baltimore has previously been shown to be effective at both reducing gun violence and improving social and community norms around the use of guns to resolve conflict. However, this study found that the program's effects have diminished over time. Although this study did not examine the factors which may have led to the program's decreased impact on gun violence, the promising findings in evaluations of the Cure Violence model in New York and Philadelphia suggest that the program would greatly benefit from increased resources and operational support, in addition to stronger connections to services for program participants. The transition of Safe Streets from the Baltimore City Health Department to the Mayor's Office, as well as plans to expand the program to additional neighborhoods, should include discussions about the disparate impacts of the program to-date, action plans for increasing support, and deeper understanding about the operations of successful program in other cities.

Finally, focused deterrence has been shown to be a model strategy for reducing gun violence in other cities. For the intervention to be successful in Baltimore, there needs to be a clear focus on the goal of violence reduction, and the program must be supported through leadership and wraparound services for those individuals reached who decide to move toward a path of nonviolence. There must also be genuine strides made to build trust between the Baltimore Police Department and community members and to engage residents on the violence-fighting strategy. The effectiveness of policing is dependent upon the public's perceived legitimacy of law enforcement's actions, and the police department must take every measure to demonstrate accountability and reconciliation in order to sustainably achieve desired levels of public safety.

# Appendix A

Figure A1: Three-Month Moving Average of Weapon Possession Arrests, 2003-2017

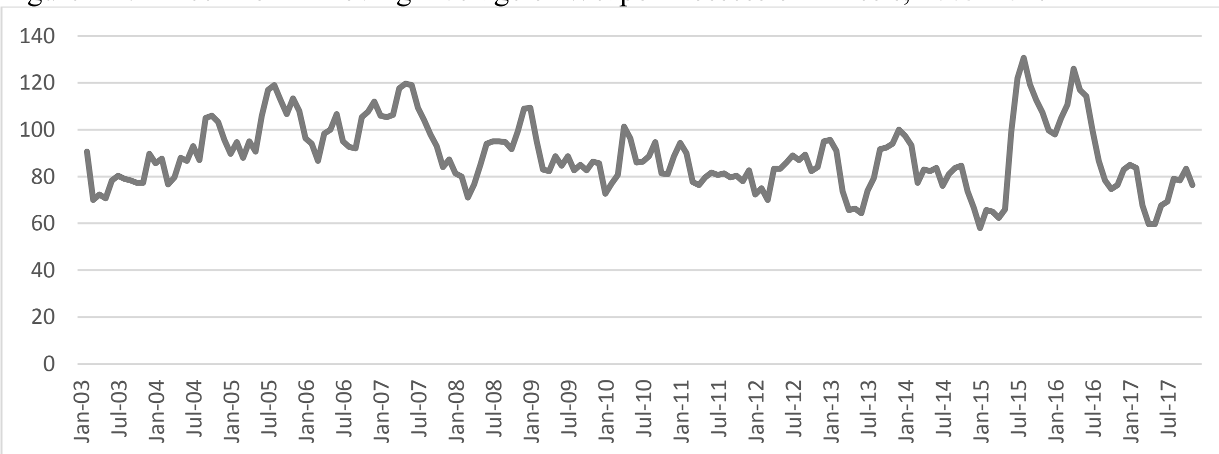


Table A1: Objective 3 Results for Major Drug Busts Stratified by Fed Involvement, Incident Rate Ratios (p-values)

| MAIN VARIABLES | Homicides | | Nonfatal Shootings | |
|---|---|---|---|---|
| Drug Poss Arrests | 0.999 | (0.852) | 1.005* | (0.042) |
| | 0.997 | (0.607) | 0.986*** | (0.001) |
| Drug Traff Arrests | 0.998 | (0.716) | 0.998 | (0.697) |
| Weapon Poss Arrests | 1.000 | (0.985) | 0.996 | (0.734) |
| Drug Bust, 1-mo effect w/ fed inv. | 1.278 | (0.467) | 1.309 | (0.340) |
| Drug Bust, 1-mo effect w/o fed inv. | 2.548 | (0.191) | 0.512 | (0.186) |
| Drug Bust, 2-mo effect w/ fed inv. | 0.914 | (0.765) | 0.936 | (0.705) |
| Drug Bust, 2-mo effect w/o fed inv. | 1.204 | (0.705) | 0.634 | (0.232) |
| Drug Bust, 3-mo effect w/ fed inv. | 0.980 | (0.927) | 0.838 | (0.291) |
| Drug Bust, 3-mo effect w/o fed inv. | 1.069 | (0.842) | 0.708 | (0.186) |
| Drug Bust, 4-mo effect w/ fed inv. | 1.051 | (0.797) | 0.869 | (0.443) |
| Drug Bust, 4-mo effect w/o fed inv. | 1.308 | (0.270) | 0.864 | (0.400) |
| Drug Bust, 5-mo effect w/ fed inv. | 1.011 | (0.945) | 0.864 | (0.359) |
| Drug Bust, 5-mo effect w/o fed inv. | 1.295 | (0.198) | 0.948 | (0.697) |
| Drug Bust, 6-mo effect w/ fed inv. | 0.960 | (0.771) | 0.874 | (0.390) |
| Drug Bust, 6-mo effect w/o fed inv. | 1.367 | (0.053) | 0.875 | (0.310) |
| Drug Bust, 9-mo effect w/ fed inv. | 1.042 | (0.750) | 0.908 | (0.409) |
| Drug Bust, 9-mo effect w/o fed inv. | 1.158 | (0.304) | 0.887 | (0.342) |
| Drug Bust, 12-mo effect w/ fed inv. | 1.078 | (0.539) | 0.911 | (0.343) |
| Drug Bust, 12-mo effect w/o fed inv. | 1.028 | (0.816) | 0.845 | (0.132) |

Exponentiated coefficients; 95% confidence intervals in brackets
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table A2: Analyses of Major Drug Busts, Including Spatial Lags (SL), Incident Rate Ratios (p-values)

| | Homicides | | Nonfatal Shootings | |
|---|---|---|---|---|
| Drug Poss Arrests | 0.999 | (0.714) | 1.005* | (0.039) |
| Drug Poss Arrests SL | 1.000 | (0.955) | 0.987** | (0.005) |
| Drug Traff Arrests | 1.001 | (0.916) | 0.998 | (0.769) |
| Drug Traff Arrests SL | 0.990 | (0.310) | 0.997 | (0.697) |
| Weapon Poss Arrests | 1.003 | (0.859) | 0.980 | (0.145) |
| Weapon Poss Arrests SL | 1.026 | (0.477) | 1.026 | (0.407) |
| Drug Busts, 1-mo effect | 2.013 | (0.200) | 1.182 | (0.481) |
| Drug Busts, 1-mo effect SL | 0.984 | (0.991) | 1.246 | (0.864) |
| Drug Busts, 2-mo effect | 1.014 | (0.974) | 0.868 | (0.315) |
| Drug Busts, 2-mo effect SL | 0.541 | (0.564) | 2.275 | (0.265) |
| Drug Busts, 3-mo effect | 1.038 | (0.897) | 0.802 | (0.061) |
| Drug Busts, 3-mo effect SL | 1.098 | (0.909) | 2.612 | (0.101) |
| Drug Busts, 4-mo effect | 1.191 | (0.460) | 0.860 | (0.304) |
| Drug Busts, 4-mo effect SL | 2.761 | (0.103) | 2.209 | (0.071) |
| Drug Busts, 5-mo effect | 1.134 | (0.564) | 0.894 | (0.409) |
| Drug Busts, 5-mo effect SL | 2.225 | (0.125) | 1.582 | (0.257) |
| Drug Busts, 6-mo effect | 1.056 | (0.784) | 0.864 | (0.279) |
| Drug Busts, 6-mo effect SL | 1.607 | (0.306) | 1.889 | (0.067) |
| Drug Busts, 9-mo effect | 1.075 | (0.591) | 0.947 | (0.599) |
| Drug Busts, 9-mo effect SL | 1.424 | (0.358) | 2.118* | (0.016) |
| Drug Busts, 12-mo effect | 1.070 | (0.560) | 0.925 | (0.380) |
| Drug Busts, 12-mo effect SL | 1.277 | (0.418) | 1.747 | (0.058) |
| Post Unrest | 1.499* | (0.015) | 1.717*** | 0.000 |
| Drug Poss Arrests*Post Unrest | 1.019 | (0.156) | 0.997 | (0.784) |
| Drug Poss Arrests*Post Unrest SL | 1.026 | (0.292) | 0.998 | (0.940) |
| Drug Traff Arrests*Post Unrest | 1.001 | (0.960) | 1.009 | (0.667) |
| Drug Traff Arrests*Post Unrest SL | 0.974 | (0.520) | 0.988 | (0.748) |
| Weapon Poss Arrests*Post Unrest | 0.971 | (0.362) | 1.063* | (0.016) |
| Weapon Poss Arrests*Post Unrest SL | 0.961 | (0.547) | 0.921 | (0.182) |
| Drug Busts, 1-mo effect*Post Unrest | 0.635 | (0.516) | 0.359 | (0.169) |
| Drug Busts, 1-mo effect*Post Unrest SL | 0.270 | (0.504) | 0.170 | (0.288) |
| Drug Busts, 2-mo effect*Post Unrest | 1.051 | (0.935) | 0.739 | (0.580) |
| Drug Busts, 2-mo effect*Post Unrest SL | 0.988 | (0.994) | 0.111* | (0.028) |
| Drug Busts, 3-mo effect*Post Unrest | 0.946 | (0.921) | 0.885 | (0.745) |
| Drug Busts, 3-mo effect*Post Unrest SL | 0.500 | (0.517) | 0.160* | (0.031) |
| Drug Busts, 4-mo effect*Post Unrest | 0.921 | (0.861) | 0.985 | (0.960) |
| Drug Busts, 4-mo effect*Post Unrest SL | 0.247 | (0.096) | 0.338 | (0.144) |
| Drug Busts, 5-mo effect*Post Unrest | 0.995 | (0.990) | 0.984 | (0.943) |
| Drug Busts, 5-mo effect*Post Unrest SL | 0.338 | (0.157) | 0.674 | (0.557) |
| Drug Busts, 6-mo effect*Post Unrest | 1.228 | (0.486) | 0.982 | (0.933) |
| Drug Busts, 6-mo effect*Post Unrest SL | 0.435 | (0.250) | 0.565 | (0.358) |
| Drug Busts, 9-mo effect*Post Unrest | 1.067 | (0.719) | 0.818 | (0.303) |
| Drug Busts, 9-mo effect*Post Unrest SL | 0.573 | (0.363) | 0.460 | (0.180) |
| Drug Busts, 12-mo effect*Post Unrest | 0.972 | (0.887) | 0.823 | (0.293) |
| Drug Busts, 12-mo effect*Post Unrest SL | 0.898 | (0.833) | 0.746 | (0.544) |

Table A3: Analyses of Drug Traff. Surges, Including Spatial Lags (SL), Incident Rate Ratios (p-values)

| | Homicides | | Nonfatal Shootings | |
|---|---|---|---|---|
| Drug Poss Arrests | 0.999 | (0.725) | 1.005* | (0.043) |
| Drug Poss Arrests SL | 1.001 | (0.918) | 0.986** | (0.003) |
| Drug Traff Arrests | 1.005 | (0.440) | 1.004 | (0.560) |
| Drug Traff Arrests SL | 0.978 | (0.106) | 1.001 | (0.929) |
| Weapon Poss Arrests | 1.002 | (0.903) | 0.979 | (0.120) |
| Weapon Poss Arrests SL | 1.029 | (0.420) | 1.025 | (0.430) |
| Traff Arrest Surge, 1-mo lag | 0.869 | (0.275) | 0.863 | (0.174) |
| Traff Arrest Surge, 1-mo lag SL | 1.434 | (0.176) | 0.879 | (0.552) |
| Traff Arrest Surge, 2-mo lag | 0.884 | (0.234) | 1.077 | (0.347) |
| Traff Arrest Surge, 2-mo lag SL | 0.734 | (0.182) | 1.109 | (0.573) |
| Traff Arrest Surge, 3-mo lag | 1.111 | (0.312) | 1.232** | (0.004) |
| Traff Arrest Surge, 3-mo lag SL | 1.001 | (0.998) | 1.075 | (0.601) |
| Traff Arrest Surge, 4-mo lag | 0.970 | (0.742) | 1.163 | (0.053) |
| Traff Arrest Surge, 4-mo lag SL | 1.285 | (0.231) | 0.942 | (0.676) |
| Traff Arrest Surge, 5-mo lag | 1.091 | (0.378) | 1.269** | (0.008) |
| Traff Arrest Surge, 5-mo lag SL | 0.995 | (0.981) | 1.001 | (0.997) |
| Traff Arrest Surge, 6-mo lag | 0.939 | (0.480) | 1.014 | (0.872) |
| Traff Arrest Surge, 6-mo lag SL | 1.065 | (0.763) | 1.100 | (0.518) |
| Traff Arrest Surge, 9-mo lag | 0.903 | (0.377) | 1.134 | (0.106) |
| Traff Arrest Surge, 9-mo lag SL | 1.090 | (0.682) | 0.808 | (0.172) |
| Traff Arrest Surge, 12-mo lag | 1.106 | (0.198) | 1.145 | (0.094) |
| Traff Arrest Surge, 12-mo lag SL | 1.389 | (0.075) | 0.853 | (0.341) |
| Post Unrest | 1.470* | (0.022) | 1.712*** | 0.000 |
| Drug Poss Arrests*Post Unrest | 1.019 | (0.156) | 0.997 | (0.804) |
| Drug Poss Arrests*Post Unrest SL | 1.024 | (0.337) | 1.000 | (0.998) |
| Drug Traff Arrests*Post Unrest | 1.005 | (0.801) | 1.014 | (0.587) |
| Drug Traff Arrests*Post Unrest SL | 0.989 | (0.811) | 0.983 | (0.676) |
| Weapon Poss Arrests*Post Unrest | 0.970 | (0.347) | 1.062* | (0.019) |
| Weapon Poss Arrests*Post Unrest SL | 0.954 | (0.483) | 0.917 | (0.165) |
| Traff Arrest Surge*Post Unrest, 1-mo lag | 0.626 | (0.308) | 0.555 | (0.065) |
| Traff Arrest Surge*Post Unrest, 1-mo lag SL | 0.499 | (0.672) | 1.155 | (0.908) |
| Traff Arrest Surge*Post Unrest, 2-mo lag | 1.850 | (0.063) | 1.191 | (0.680) |
| Traff Arrest Surge*Post Unrest, 2-mo lag SL | 0.269 | (0.470) | 0.592 | (0.639) |
| Traff Arrest Surge*Post Unrest, 3-mo lag | 0.546 | (0.091) | 1.054 | (0.857) |
| Traff Arrest Surge*Post Unrest, 3-mo lag SL | 2.236 | (0.522) | 2.458 | (0.421) |
| Traff Arrest Surge*Post Unrest, 4-mo lag | 1.476 | (0.314) | 1.439 | (0.354) |
| Traff Arrest Surge*Post Unrest, 4-mo lag SL | 0.424 | (0.504) | 1.366 | (0.765) |
| Traff Arrest Surge*Post Unrest, 5-mo lag | 0.208 | (0.194) | 0.412*** | 0.000 |
| Traff Arrest Surge*Post Unrest, 5-mo lag SL | 0.584 | (0.766) | 0.959 | (0.972) |
| Traff Arrest Surge*Post Unrest, 6-mo lag | 1.935 | (0.087) | 3.024*** | 0.000 |
| Traff Arrest Surge*Post Unrest, 6-mo lag SL | 1.335 | (0.828) | 0.689 | (0.638) |
| Traff Arrest Surge*Post Unrest, 9-mo lag | 1.402 | (0.330) | 1.027 | (0.929) |
| Traff Arrest Surge*Post Unrest, 9-mo lag SL | 1.223 | (0.850) | 3.703* | (0.042) |
| Traff Arrest Surge*Post Unrest, 12-mo lag | 1.116 | (0.708) | 0.798 | (0.313) |
| Traff Arrest Surge*Post Unrest, 12-mo lag SL | 1.045 | (0.957) | 3.913 | (0.098) |

# Appendix B

Figure B1: Synthetic Control Analyses for Homicides in Safe Streets Police Posts

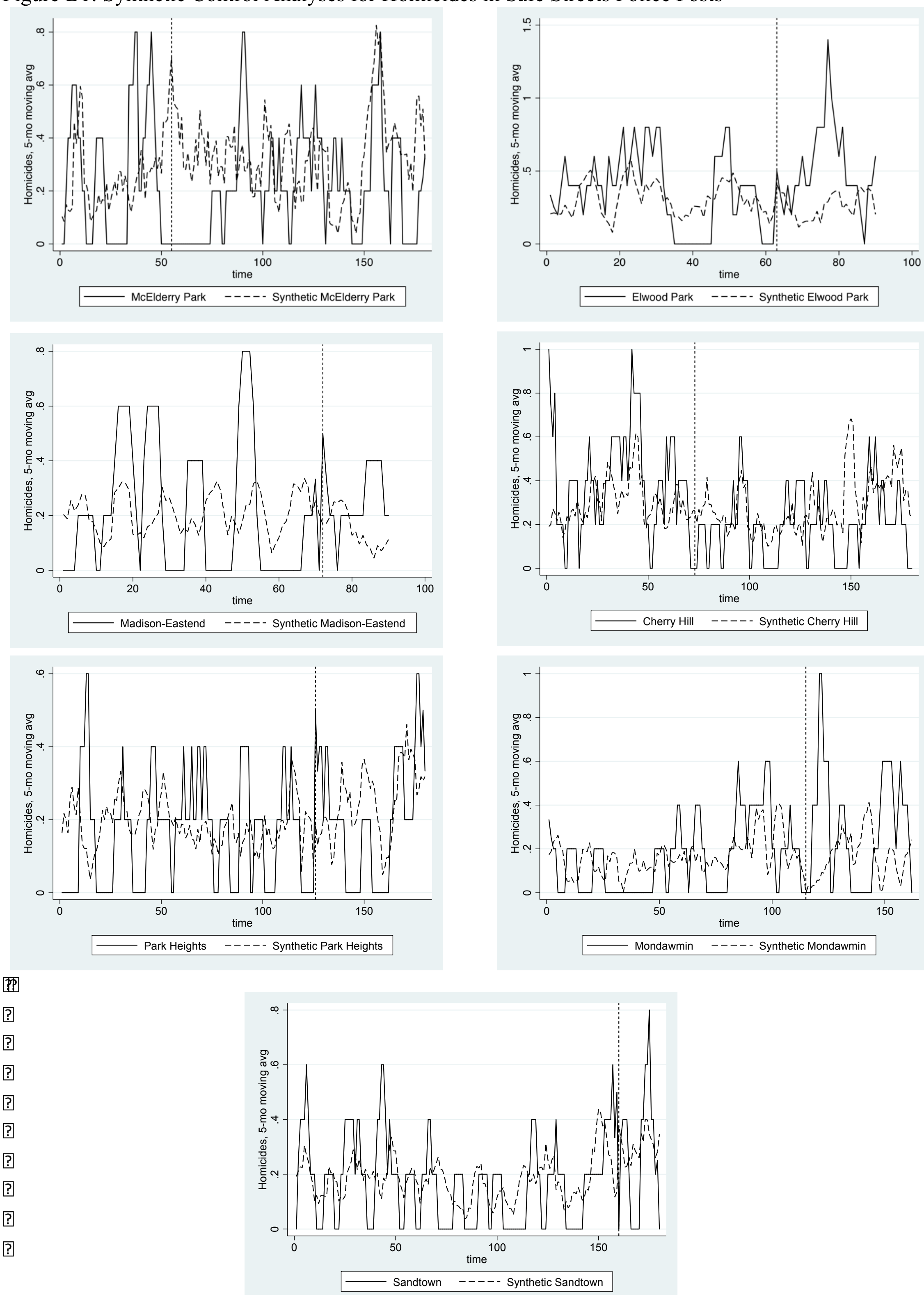


Figure B2: Synthetic Control Analyses for Nonfatal Shootings in Safe Streets Police Posts

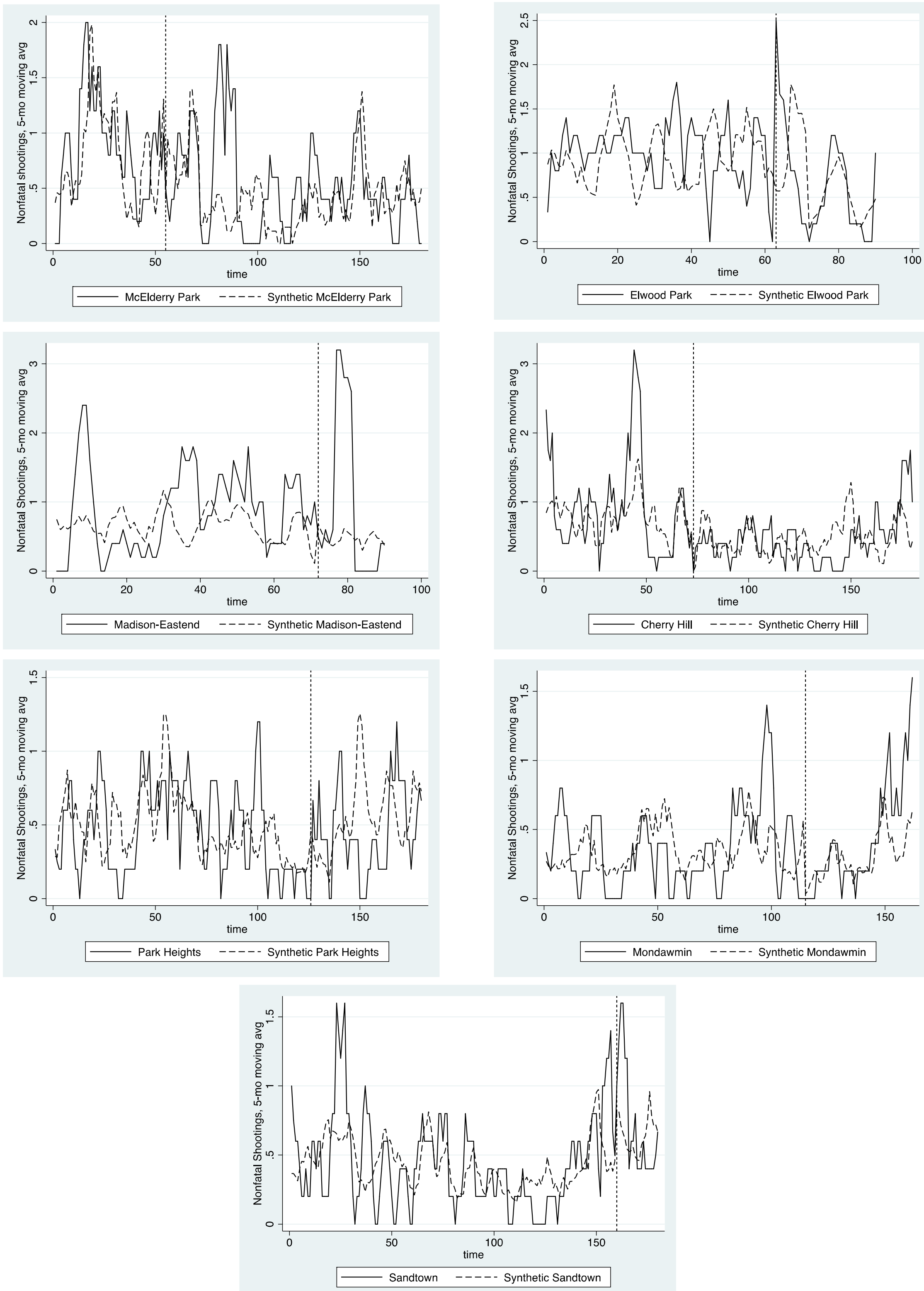


Table B3: Predictor Balances and Non-Zero Weights from Synthetic Control Analyses

McElderry Park - Homicides

Predictor Balance

| | Treated | Synthetic |
|---|---|---|
| homicide_yr_avg(12) | 0.33 | 0.31 |
| homicide_yr_avg(24) | 0.17 | 0.12 |
| homicide_yr_avg(36) | 0.25 | 0.21 |
| homicide_yr_avg(48) | 0.42 | 0.27 |
| shooting_yr_avg(12) | 0.50 | 0.75 |
| shooting_yr_avg(24) | 1.58 | 1.03 |
| shooting_yr_avg(36) | 0.83 | 0.82 |
| shooting_yr_avg(48) | 0.50 | 0.78 |
| wpnposs_yr_avg(12) | 2.08 | 1.02 |
| wpnposs_yr_avg(24) | 2.50 | 1.35 |
| wpnposs_yr_avg(36) | 1.25 | 1.40 |
| wpnposs_yr_avg(48) | 1.83 | 1.25 |
| drugposs_yr_avg(12) | 28.00 | 25.69 |
| drugposs_yr_avg(24) | 26.08 | 19.97 |
| drugposs_yr_avg(36) | 29.75 | 24.91 |
| drugposs_yr_avg(48) | 29.42 | 31.83 |
| drugtraff_yr_avg(12) | 30.50 | 18.48 |
| drugtraff_yr_avg(24) | 17.33 | 13.84 |
| drugtraff_yr_avg(36) | 10.83 | 10.06 |
| drugtraff_yr_avg(48) | 10.67 | 12.05 |

Non-Zero Weights for Synthetic Control

| Post | Weight |
|---|---|
| 132 | 0.18 |
| 311 | 0.49 |
| 313 | 0.02 |
| 314 | 0.01 |
| 315 | 0.08 |
| 323 | 0.03 |

Elwood Park - Homicides

Predictor Balance

| | Treated | Synthetic |
|---|---|---|
| homicide_yr_avg(12) | 0.33 | 0.31 |
| homicide_yr_avg(24) | 0.50 | 0.32 |
| homicide_yr_avg(36) | 0.50 | 0.34 |
| homicide_yr_avg(48) | 0.25 | 0.25 |
| homicide_yr_avg(60) | 0.25 | 0.35 |
| shooting_yr_avg(12) | 1.00 | 0.80 |
| shooting_yr_avg(24) | 1.17 | 0.66 |
| shooting_yr_avg(36) | 1.00 | 0.70 |
| shooting_yr_avg(48) | 1.08 | 0.69 |
| shooting_yr_avg(60) | 1.00 | 0.84 |
| wpnposs_yr_avg(12) | 1.67 | 0.83 |
| wpnposs_yr_avg(24) | 0.92 | 1.00 |
| wpnposs_yr_avg(36) | 2.33 | 1.30 |
| wpnposs_yr_avg(48) | 2.08 | 1.18 |
| wpnposs_yr_avg(60) | 1.25 | 1.48 |
| drugposs_yr_avg(12) | 13.33 | 14.65 |
| drugposs_yr_avg(24) | 20.08 | 17.88 |
| drugposs_yr_avg(36) | 28.67 | 23.60 |
| drugposs_yr_avg(48) | 26.25 | 19.34 |
| drugposs_yr_avg(60) | 22.92 | 20.12 |
| drugtraff_yr_avg(12) | 10.58 | 10.49 |
| drugtraff_yr_avg(24) | 16.08 | 11.15 |
| drugtraff_yr_avg(36) | 11.92 | 9.73 |
| drugtraff_yr_avg(48) | 9.42 | 9.01 |
| drugtraff_yr_avg(60) | 7.42 | 8.73 |

Non-Zero Weights for Synthetic Control

| Post | Weight |
|---|---|
| 132 | 0.02 |
| 322 | 0.17 |
| 333 | 0.24 |
| 613 | 0.04 |
| 725 | 0.16 |
| 732 | 0.20 |
| 814 | 0.12 |

Madison-Eastend - Homicides

Predictor Balance

| | Treated | Synthetic |
|---|---|---|
| homicide_yr_avg(12) | 0.20 | 0.20 |
| homicide_yr_avg(24) | 0.20 | 0.20 |
| homicide_yr_avg(36) | 0.20 | 0.20 |
| homicide_yr_avg(48) | 0.20 | 0.20 |
| homicide_yr_avg(60) | 0.20 | 0.20 |
| shooting_yr_avg(12) | 0.71 | 0.60 |
| shooting_yr_avg(24) | 0.71 | 0.66 |
| shooting_yr_avg(36) | 0.71 | 0.68 |
| shooting_yr_avg(48) | 0.71 | 0.74 |
| shooting_yr_avg(60) | 0.71 | 0.65 |
| wpnposs_yr_avg(12) | 0.99 | 0.92 |
| wpnposs_yr_avg(24) | 0.99 | 0.91 |
| wpnposs_yr_avg(36) | 0.99 | 0.97 |
| wpnposs_yr_avg(48) | 0.99 | 1.04 |
| wpnposs_yr_avg(60) | 0.99 | 1.06 |
| drugposs_yr_avg(12) | 12.25 | 11.56 |
| drugposs_yr_avg(24) | 12.25 | 11.82 |
| drugposs_yr_avg(36) | 12.25 | 12.58 |
| drugposs_yr_avg(48) | 12.25 | 12.60 |
| drugposs_yr_avg(60) | 12.25 | 13.21 |
| drugtraff_yr_avg(12) | 6.58 | 7.40 |
| drugtraff_yr_avg(24) | 6.58 | 7.50 |
| drugtraff_yr_avg(36) | 6.58 | 6.48 |
| drugtraff_yr_avg(48) | 6.58 | 6.23 |
| drugtraff_yr_avg(60) | 6.58 | 6.37 |

Non-Zero Weights for Synthetic Control

| Post | Weight |
|---|---|
| 142 | 0.02 |
| 234 | 0.01 |
| 313 | 0.09 |
| 321 | 0.10 |
| 331 | 0.04 |
| 412 | 0.22 |
| 426 | 0.05 |
| 436 | 0.19 |
| 714 | 0.02 |
| 725 | 0.01 |
| 735 | 0.05 |
| 815 | 0.12 |
| 816 | 0.02 |
| 835 | 0.02 |
| 934 | 0.05 |

Cherry Hill - Homicides

Predictor Balance

| | Treated | Synthetic |
|---|---|---|
| homicide_yr_avg(12) | 0.33 | 0.22 |
| homicide_yr_avg(24) | 0.42 | 0.31 |
| homicide_yr_avg(36) | 0.42 | 0.34 |
| homicide_yr_avg(48) | 0.67 | 0.41 |
| homicide_yr_avg(60) | 0.17 | 0.25 |
| homicide_yr_avg(72) | 0.42 | 0.30 |
| shooting_yr_avg(12) | 1.00 | 0.75 |
| shooting_yr_avg(24) | 1.00 | 0.71 |
| shooting_yr_avg(36) | 0.67 | 0.88 |
| shooting_yr_avg(48) | 2.00 | 0.67 |
| shooting_yr_avg(60) | 0.17 | 0.58 |
| shooting_yr_avg(72) | 0.67 | 0.47 |
| wpnposs_yr_avg(12) | 0.92 | 0.85 |
| wpnposs_yr_avg(24) | 1.42 | 1.13 |
| wpnposs_yr_avg(36) | 0.92 | 1.32 |
| wpnposs_yr_avg(48) | 1.33 | 0.99 |
| wpnposs_yr_avg(60) | 1.08 | 1.22 |
| wpnposs_yr_avg(72) | 1.33 | 0.91 |
| drugposs_yr_avg(12) | 11.08 | 13.77 |
| drugposs_yr_avg(24) | 15.17 | 17.61 |
| drugposs_yr_avg(36) | 15.67 | 15.77 |
| drugposs_yr_avg(48) | 19.50 | 15.89 |
| drugposs_yr_avg(60) | 21.33 | 13.68 |
| drugposs_yr_avg(72) | 12.17 | 15.34 |
| drugtraff_yr_avg(12) | 5.42 | 7.42 |
| drugtraff_yr_avg(24) | 8.67 | 6.93 |
| drugtraff_yr_avg(36) | 6.00 | 6.23 |
| drugtraff_yr_avg(48) | 4.92 | 4.80 |
| drugtraff_yr_avg(60) | 1.42 | 3.97 |
| drugtraff_yr_avg(72) | 3.67 | 4.77 |

Non-Zero Weights for Synthetic Control

| Post | Weight |
|---|---|
| 333 | 0.04 |
| 412 | 0.36 |
| 531 | 0.18 |
| 613 | 0.13 |
| 723 | 0.07 |
| 725 | 0.13 |
| 735 | 0.10 |

Mondawmin - Homicides

Predictor Balance

| | Treated | Synthetic |
|---|---|---|
| homicide_yr_avg(12) | 0.17 | 0.15 |
| homicide_yr_avg(24) | 0.08 | 0.13 |
| homicide_yr_avg(36) | 0.00 | 0.09 |
| homicide_yr_avg(48) | 0.00 | 0.13 |
| homicide_yr_avg(60) | 0.25 | 0.17 |
| homicide_yr_avg(72) | 0.17 | 0.15 |
| homicide_yr_avg(84) | 0.08 | 0.16 |
| homicide_yr_avg(96) | 0.33 | 0.27 |
| homicide_yr_avg(108) | 0.33 | 0.24 |
| shooting_yr_avg(12) | 0.50 | 0.20 |
| shooting_yr_avg(24) | 0.33 | 0.49 |
| shooting_yr_avg(36) | 0.00 | 0.32 |
| shooting_yr_avg(48) | 0.42 | 0.53 |
| shooting_yr_avg(60) | 0.25 | 0.58 |
| shooting_yr_avg(72) | 0.17 | 0.34 |
| shooting_yr_avg(84) | 0.25 | 0.35 |
| shooting_yr_avg(96) | 0.67 | 0.58 |
| shooting_yr_avg(108) | 0.67 | 0.26 |
| wpnposs_yr_avg(12) | 0.17 | 0.47 |
| wpnposs_yr_avg(24) | 0.83 | 0.67 |
| wpnposs_yr_avg(36) | 0.92 | 0.57 |
| wpnposs_yr_avg(48) | 0.67 | 0.61 |
| wpnposs_yr_avg(60) | 0.75 | 0.70 |
| wpnposs_yr_avg(72) | 0.58 | 0.87 |
| wpnposs_yr_avg(84) | 0.67 | 0.81 |
| wpnposs_yr_avg(96) | 1.08 | 0.76 |
| wpnposs_yr_avg(108) | 1.00 | 0.70 |
| drugposs_yr_avg(12) | 2.92 | 6.62 |
| drugposs_yr_avg(24) | 3.58 | 5.57 |
| drugposs_yr_avg(36) | 4.42 | 7.29 |
| drugposs_yr_avg(48) | 8.33 | 11.27 |
| drugposs_yr_avg(60) | 15.50 | 11.87 |
| drugposs_yr_avg(72) | 15.58 | 13.56 |
| drugposs_yr_avg(84) | 9.75 | 11.23 |
| drugposs_yr_avg(96) | 8.00 | 10.31 |
| drugposs_yr_avg(108) | 8.50 | 9.49 |
| drugtraff_yr_avg(12) | 2.08 | 3.71 |
| drugtraff_yr_avg(24) | 2.17 | 3.79 |
| drugtraff_yr_avg(36) | 1.17 | 3.25 |
| drugtraff_yr_avg(48) | 2.42 | 4.70 |
| drugtraff_yr_avg(60) | 4.83 | 4.20 |
| drugtraff_yr_avg(72) | 5.42 | 5.50 |
| drugtraff_yr_avg(84) | 9.08 | 5.87 |
| drugtraff_yr_avg(96) | 5.25 | 4.50 |
| drugtraff_yr_avg(108) | 5.00 | 5.00 |

Non-Zero Weights for Synthetic Control

| Post | Weight |
|---|---|
| 113 | 0.14 |
| 132 | 0.07 |
| 211 | 0.10 |
| 312 | 0.05 |
| 332 | 0.10 |
| 411 | 0.04 |
| 424 | 0.21 |
| 425 | 0.02 |
| 436 | 0.28 |

Lower Park Heights - Homicides

Predictor Balance

| | Treated | Synthetic |
|---|---|---|
| homicide_yr_avg(12) | 0.17 | 0.22 |
| homicide_yr_avg(24) | 0.08 | 0.13 |
| homicide_yr_avg(36) | 0.17 | 0.23 |
| homicide_yr_avg(48) | 0.17 | 0.19 |
| homicide_yr_avg(60) | 0.17 | 0.24 |
| homicide_yr_avg(72) | 0.25 | 0.17 |
| homicide_yr_avg(84) | 0.17 | 0.16 |
| homicide_yr_avg(96) | 0.17 | 0.16 |
| homicide_yr_avg(108) | 0.08 | 0.14 |
| homicide_yr_avg(120) | 0.25 | 0.23 |
| shooting_yr_avg(12) | 0.42 | 0.69 |
| shooting_yr_avg(24) | 0.67 | 0.42 |
| shooting_yr_avg(36) | 0.08 | 0.46 |
| shooting_yr_avg(48) | 0.58 | 0.62 |
| shooting_yr_avg(60) | 0.83 | 0.69 |
| shooting_yr_avg(72) | 0.58 | 0.55 |
| shooting_yr_avg(84) | 0.42 | 0.31 |
| shooting_yr_avg(96) | 0.50 | 0.32 |
| shooting_yr_avg(108) | 0.58 | 0.44 |
| shooting_yr_avg(120) | 0.08 | 0.24 |
| wpnposs_yr_avg(12) | 1.08 | 0.75 |
| wpnposs_yr_avg(24) | 1.17 | 0.97 |
| wpnposs_yr_avg(36) | 0.67 | 0.95 |
| wpnposs_yr_avg(48) | 1.08 | 1.15 |
| wpnposs_yr_avg(60) | 1.50 | 1.17 |
| wpnposs_yr_avg(72) | 1.58 | 1.08 |
| wpnposs_yr_avg(84) | 0.67 | 1.15 |
| wpnposs_yr_avg(96) | 0.42 | 0.89 |
| wpnposs_yr_avg(108) | 0.83 | 0.82 |
| wpnposs_yr_avg(120) | 0.17 | 0.97 |
| drugposs_yr_avg(12) | 15.83 | 15.66 |
| drugposs_yr_avg(24) | 33.67 | 19.40 |
| drugposs_yr_avg(36) | 28.17 | 23.04 |
| drugposs_yr_avg(48) | 24.58 | 23.87 |
| drugposs_yr_avg(60) | 17.83 | 18.93 |
| drugposs_yr_avg(72) | 22.17 | 21.65 |
| drugposs_yr_avg(84) | 16.83 | 16.30 |
| drugposs_yr_avg(96) | 18.67 | 16.49 |
| drugposs_yr_avg(108) | 15.08 | 14.50 |
| drugposs_yr_avg(120) | 15.83 | 15.57 |
| drugtraff_yr_avg(12) | 11.08 | 10.86 |
| drugtraff_yr_avg(24) | 10.75 | 10.08 |
| drugtraff_yr_avg(36) | 7.17 | 6.08 |
| drugtraff_yr_avg(48) | 7.17 | 7.73 |
| drugtraff_yr_avg(60) | 4.17 | 6.41 |
| drugtraff_yr_avg(72) | 4.33 | 6.65 |
| drugtraff_yr_avg(84) | 4.17 | 5.57 |
| drugtraff_yr_avg(96) | 6.92 | 5.67 |
| drugtraff_yr_avg(108) | 6.17 | 5.84 |
| drugtraff_yr_avg(120) | 3.17 | 4.55 |

Non-Zero Weights for Synthetic Control

| Post | Weight |
|---|---|
| 124 | 0.01 |
| 412 | 0.00 |
| 531 | 0.06 |
| 611 | 0.10 |
| 614 | 0.15 |
| 615 | 0.08 |
| 715 | 0.14 |
| 732 | 0.16 |
| 814 | 0.12 |
| 815 | 0.13 |
| 834 | 0.06 |

Sandtown - Homicides

Predictor Balance

| | Treated | Synthetic |
|---|---|---|
| homicide_yr_avg(12) | 0.25 | 0.20 |
| homicide_yr_avg(24) | 0.08 | 0.15 |
| homicide_yr_avg(36) | 0.33 | 0.21 |
| homicide_yr_avg(48) | 0.25 | 0.22 |
| homicide_yr_avg(60) | 0.17 | 0.20 |
| homicide_yr_avg(72) | 0.17 | 0.19 |
| homicide_yr_avg(84) | 0.08 | 0.11 |
| homicide_yr_avg(96) | 0.08 | 0.16 |
| homicide_yr_avg(108) | 0.08 | 0.09 |
| homicide_yr_avg(120) | 0.17 | 0.18 |
| homicide_yr_avg(132) | 0.17 | 0.21 |
| homicide_yr_avg(144) | 0.00 | 0.11 |
| homicide_yr_avg(156) | 0.33 | 0.33 |
| shooting_yr_avg(12) | 0.42 | 0.49 |
| shooting_yr_avg(24) | 0.58 | 0.57 |
| shooting_yr_avg(36) | 0.75 | 0.41 |
| shooting_yr_avg(48) | 0.58 | 0.47 |
| shooting_yr_avg(60) | 0.17 | 0.48 |
| shooting_yr_avg(72) | 0.58 | 0.57 |
| shooting_yr_avg(84) | 0.42 | 0.29 |
| shooting_yr_avg(96) | 0.33 | 0.27 |
| shooting_yr_avg(108) | 0.33 | 0.24 |
| shooting_yr_avg(120) | 0.17 | 0.29 |
| shooting_yr_avg(132) | 0.08 | 0.27 |
| shooting_yr_avg(144) | 0.50 | 0.26 |
| shooting_yr_avg(156) | 0.75 | 0.56 |
| wpnposs_yr_avg(12) | 0.92 | 0.57 |
| wpnposs_yr_avg(24) | 1.33 | 0.71 |
| wpnposs_yr_avg(36) | 1.08 | 0.90 |
| wpnposs_yr_avg(48) | 0.50 | 0.73 |
| wpnposs_yr_avg(60) | 0.75 | 0.83 |
| wpnposs_yr_avg(72) | 0.58 | 0.80 |
| wpnposs_yr_avg(84) | 0.67 | 0.78 |
| wpnposs_yr_avg(96) | 0.50 | 0.57 |
| wpnposs_yr_avg(108) | 0.67 | 0.66 |
| wpnposs_yr_avg(120) | 0.33 | 0.64 |
| wpnposs_yr_avg(132) | 0.67 | 0.66 |
| wpnposs_yr_avg(144) | 0.42 | 0.68 |
| wpnposs_yr_avg(156) | 0.58 | 0.75 |
| drugposs_yr_avg(12) | 11.00 | 12.67 |
| drugposs_yr_avg(24) | 11.00 | 11.65 |
| drugposs_yr_avg(36) | 12.08 | 15.07 |
| drugposs_yr_avg(48) | 13.42 | 19.19 |
| drugposs_yr_avg(60) | 25.75 | 19.83 |
| drugposs_yr_avg(72) | 25.67 | 21.98 |
| drugposs_yr_avg(84) | 13.75 | 15.14 |
| drugposs_yr_avg(96) | 12.50 | 14.75 |
| drugposs_yr_avg(108) | 16.33 | 13.36 |
| drugposs_yr_avg(120) | 19.25 | 14.07 |
| drugposs_yr_avg(132) | 11.17 | 13.44 |
| drugposs_yr_avg(144) | 8.50 | 9.05 |
| drugposs_yr_avg(156) | 3.75 | 4.02 |
| drugtraff_yr_avg(12) | 7.42 | 8.34 |
| drugtraff_yr_avg(24) | 7.92 | 7.13 |
| drugtraff_yr_avg(36) | 4.75 | 5.27 |
| drugtraff_yr_avg(48) | 4.17 | 6.68 |
| drugtraff_yr_avg(60) | 7.75 | 6.40 |
| drugtraff_yr_avg(72) | 6.58 | 7.26 |
| drugtraff_yr_avg(84) | 5.92 | 6.57 |
| drugtraff_yr_avg(96) | 5.92 | 5.67 |
| drugtraff_yr_avg(108) | 7.50 | 6.02 |
| drugtraff_yr_avg(120) | 4.25 | 4.72 |
| drugtraff_yr_avg(132) | 3.08 | 4.56 |
| drugtraff_yr_avg(144) | 4.00 | 3.55 |
| drugtraff_yr_avg(156) | 3.58 | 2.89 |

Non-Zero Weights for Synthetic Control

| Post | Weight |
|---|---|
| 113 | 0.02 |
| 131 | 0.02 |
| 132 | 0.08 |
| 323 | 0.08 |
| 331 | 0.04 |
| 415 | 0.02 |
| 421 | 0.11 |
| 525 | 0.00 |
| 536 | 0.11 |
| 611 | 0.01 |
| 615 | 0.11 |
| 623 | 0.06 |
| 634 | 0.12 |
| 712 | 0.03 |
| 725 | 0.02 |
| 732 | 0.11 |
| 733 | 0.07 |

McElderry Park – Nonfatal Shootings

Predictor Balance

| | Treated | Synthetic |
|---|---|---|
| homicide_yr_avg(12) | 0.33 | 0.24 |
| homicide_yr_avg(24) | 0.17 | 0.16 |
| homicide_yr_avg(36) | 0.25 | 0.16 |
| homicide_yr_avg(48) | 0.42 | 0.29 |
| shooting_yr_avg(12) | 0.50 | 0.49 |
| shooting_yr_avg(24) | 1.58 | 1.33 |
| shooting_yr_avg(36) | 0.83 | 0.90 |
| shooting_yr_avg(48) | 0.50 | 0.52 |
| wpnposs_yr_avg(12) | 2.08 | 0.87 |
| wpnposs_yr_avg(24) | 2.50 | 1.27 |
| wpnposs_yr_avg(36) | 1.25 | 1.28 |
| wpnposs_yr_avg(48) | 1.83 | 0.90 |
| drugposs_yr_avg(12) | 28.00 | 25.52 |
| drugposs_yr_avg(24) | 26.08 | 18.37 |
| drugposs_yr_avg(36) | 29.75 | 22.66 |
| drugposs_yr_avg(48) | 29.42 | 30.00 |
| drugtraff_yr_avg(12) | 30.50 | 18.45 |
| drugtraff_yr_avg(24) | 17.33 | 16.48 |
| drugtraff_yr_avg(36) | 10.83 | 11.66 |
| drugtraff_yr_avg(48) | 10.67 | 11.68 |

Non-Zero Weights for Synthetic Control

| Post | Weight |
|---|---|
| 132 | 0.19 |
| 311 | 0.37 |
| 323 | 0.44 |

Elwood Park – Nonfatal Shootings

Predictor Balance

| | Treated | Synthetic |
|---|---|---|
| homicide_yr_avg(12) | 0.33 | 0.33 |
| homicide_yr_avg(24) | 0.50 | 0.21 |
| homicide_yr_avg(36) | 0.50 | 0.16 |
| homicide_yr_avg(48) | 0.25 | 0.28 |
| homicide_yr_avg(60) | 0.25 | 0.43 |
| shooting_yr_avg(12) | 1.00 | 0.91 |
| shooting_yr_avg(24) | 1.17 | 1.05 |
| shooting_yr_avg(36) | 1.00 | 0.91 |
| shooting_yr_avg(48) | 1.08 | 0.94 |
| shooting_yr_avg(60) | 1.00 | 1.06 |
| wpnposs_yr_avg(12) | 1.67 | 0.96 |
| wpnposs_yr_avg(24) | 0.92 | 1.30 |
| wpnposs_yr_avg(36) | 2.33 | 1.58 |
| wpnposs_yr_avg(48) | 2.08 | 1.39 |
| wpnposs_yr_avg(60) | 1.25 | 1.40 |
| drugposs_yr_avg(12) | 13.33 | 18.45 |
| drugposs_yr_avg(24) | 20.08 | 14.95 |
| drugposs_yr_avg(36) | 28.67 | 19.64 |
| drugposs_yr_avg(48) | 26.25 | 20.91 |
| drugposs_yr_avg(60) | 22.92 | 18.99 |
| drugtraff_yr_avg(12) | 10.58 | 15.60 |
| drugtraff_yr_avg(24) | 16.08 | 12.42 |
| drugtraff_yr_avg(36) | 11.92 | 10.23 |
| drugtraff_yr_avg(48) | 9.42 | 9.00 |
| drugtraff_yr_avg(60) | 7.42 | 8.98 |

Non-Zero Weights for Synthetic Control

| Post | Weight |
|---|---|
| 132 | 0.02 |
| 311 | 0.50 |
| 321 | 0.02 |
| 333 | 0.15 |
| 725 | 0.23 |
| 814 | 0.04 |
| 922 | 0.03 |

Madison-Eastend – Nonfatal Shootings

Predictor Balance

| | Treated | Synthetic |
|---|---|---|
| homicide_yr_avg(12) | 0.20 | 0.20 |
| homicide_yr_avg(24) | 0.20 | 0.20 |
| homicide_yr_avg(36) | 0.20 | 0.20 |
| homicide_yr_avg(48) | 0.20 | 0.20 |
| homicide_yr_avg(60) | 0.20 | 0.20 |
| shooting_yr_avg(12) | 0.71 | 0.60 |
| shooting_yr_avg(24) | 0.71 | 0.66 |
| shooting_yr_avg(36) | 0.71 | 0.68 |
| shooting_yr_avg(48) | 0.71 | 0.74 |
| shooting_yr_avg(60) | 0.71 | 0.65 |
| wpnposs_yr_avg(12) | 0.99 | 0.92 |
| wpnposs_yr_avg(24) | 0.99 | 0.91 |
| wpnposs_yr_avg(36) | 0.99 | 0.97 |
| wpnposs_yr_avg(48) | 0.99 | 1.04 |
| wpnposs_yr_avg(60) | 0.99 | 1.06 |
| drugposs_yr_avg(12) | 12.25 | 11.56 |
| drugposs_yr_avg(24) | 12.25 | 11.82 |
| drugposs_yr_avg(36) | 12.25 | 12.58 |
| drugposs_yr_avg(48) | 12.25 | 12.60 |
| drugposs_yr_avg(60) | 12.25 | 13.21 |
| drugtraff_yr_avg(12) | 6.58 | 7.40 |
| drugtraff_yr_avg(24) | 6.58 | 7.50 |
| drugtraff_yr_avg(36) | 6.58 | 6.48 |
| drugtraff_yr_avg(48) | 6.58 | 6.23 |
| drugtraff_yr_avg(60) | 6.58 | 6.37 |

Non-Zero Weights for Synthetic Control

| Post | Weight |
|---|---|
| 131 | 0.01 |
| 142 | 0.05 |
| 212 | 0.05 |
| 234 | 0.02 |
| 311 | 0.03 |
| 313 | 0.11 |
| 321 | 0.08 |
| 331 | 0.03 |
| 412 | 0.14 |
| 436 | 0.18 |
| 714 | 0.04 |
| 725 | 0.02 |
| 735 | 0.10 |
| 815 | 0.12 |
| 816 | 0.01 |
| 835 | 0.03 |

Cherry Hill – Nonfatal Shootings

Predictor Balance

| | Treated | Synthetic |
|---|---|---|
| homicide_yr_avg(12) | 0.33 | 0.31 |
| homicide_yr_avg(24) | 0.42 | 0.28 |
| homicide_yr_avg(36) | 0.42 | 0.21 |
| homicide_yr_avg(48) | 0.67 | 0.25 |
| homicide_yr_avg(60) | 0.17 | 0.23 |
| homicide_yr_avg(72) | 0.42 | 0.20 |
| shooting_yr_avg(12) | 1.00 | 0.98 |
| shooting_yr_avg(24) | 1.00 | 0.67 |
| shooting_yr_avg(36) | 0.67 | 0.70 |
| shooting_yr_avg(48) | 2.00 | 1.13 |
| shooting_yr_avg(60) | 0.17 | 0.64 |
| shooting_yr_avg(72) | 0.67 | 0.61 |
| wpnposs_yr_avg(12) | 0.92 | 0.74 |
| wpnposs_yr_avg(24) | 1.42 | 0.95 |
| wpnposs_yr_avg(36) | 0.92 | 0.94 |
| wpnposs_yr_avg(48) | 1.33 | 1.09 |
| wpnposs_yr_avg(60) | 1.08 | 0.94 |
| wpnposs_yr_avg(72) | 1.33 | 1.13 |
| drugposs_yr_avg(12) | 11.08 | 8.91 |
| drugposs_yr_avg(24) | 15.17 | 12.75 |
| drugposs_yr_avg(36) | 15.67 | 13.30 |
| drugposs_yr_avg(48) | 19.50 | 15.68 |
| drugposs_yr_avg(60) | 21.33 | 14.27 |
| drugposs_yr_avg(72) | 12.17 | 14.75 |
| drugtraff_yr_avg(12) | 5.42 | 5.39 |
| drugtraff_yr_avg(24) | 8.67 | 6.91 |
| drugtraff_yr_avg(36) | 6.00 | 5.53 |
| drugtraff_yr_avg(48) | 4.92 | 5.63 |
| drugtraff_yr_avg(60) | 1.42 | 5.97 |
| drugtraff_yr_avg(72) | 3.67 | 5.81 |

Non-Zero Weights for Synthetic Control

| Post | Weight |
|---|---|
| 132 | 0.04 |
| 725 | 0.41 |
| 922 | 0.35 |

Mondawmin – Nonfatal Shootings

Predictor Balance

| | Treated | Synthetic |
|---|---|---|
| homicide_yr_avg(12) | 0.17 | 0.12 |
| homicide_yr_avg(24) | 0.08 | 0.17 |
| homicide_yr_avg(36) | 0.00 | 0.11 |
| homicide_yr_avg(48) | 0.00 | 0.19 |
| homicide_yr_avg(60) | 0.25 | 0.23 |
| homicide_yr_avg(72) | 0.17 | 0.15 |
| homicide_yr_avg(84) | 0.08 | 0.19 |
| homicide_yr_avg(96) | 0.33 | 0.23 |
| homicide_yr_avg(108) | 0.33 | 0.16 |
| shooting_yr_avg(12) | 0.50 | 0.26 |
| shooting_yr_avg(24) | 0.33 | 0.38 |
| shooting_yr_avg(36) | 0.00 | 0.20 |
| shooting_yr_avg(48) | 0.42 | 0.48 |
| shooting_yr_avg(60) | 0.25 | 0.50 |
| shooting_yr_avg(72) | 0.17 | 0.26 |
| shooting_yr_avg(84) | 0.25 | 0.32 |
| shooting_yr_avg(96) | 0.67 | 0.49 |
| shooting_yr_avg(108) | 0.67 | 0.34 |
| wpnposs_yr_avg(12) | 0.17 | 0.42 |
| wpnposs_yr_avg(24) | 0.83 | 0.70 |
| wpnposs_yr_avg(36) | 0.92 | 0.70 |
| wpnposs_yr_avg(48) | 0.67 | 0.50 |
| wpnposs_yr_avg(60) | 0.75 | 0.72 |
| wpnposs_yr_avg(72) | 0.58 | 0.84 |
| wpnposs_yr_avg(84) | 0.67 | 0.75 |
| wpnposs_yr_avg(96) | 1.08 | 0.73 |
| wpnposs_yr_avg(108) | 1.00 | 0.65 |
| drugposs_yr_avg(12) | 2.92 | 6.08 |
| drugposs_yr_avg(24) | 3.58 | 5.55 |
| drugposs_yr_avg(36) | 4.42 | 8.04 |
| drugposs_yr_avg(48) | 8.33 | 10.55 |
| drugposs_yr_avg(60) | 15.50 | 10.93 |
| drugposs_yr_avg(72) | 15.58 | 12.26 |
| drugposs_yr_avg(84) | 9.75 | 10.69 |
| drugposs_yr_avg(96) | 8.00 | 8.91 |
| drugposs_yr_avg(108) | 8.50 | 8.57 |
| drugtraff_yr_avg(12) | 2.08 | 3.71 |
| drugtraff_yr_avg(24) | 2.17 | 4.25 |
| drugtraff_yr_avg(36) | 1.17 | 3.65 |
| drugtraff_yr_avg(48) | 2.42 | 4.04 |
| drugtraff_yr_avg(60) | 4.83 | 4.38 |
| drugtraff_yr_avg(72) | 5.42 | 5.95 |
| drugtraff_yr_avg(84) | 9.08 | 5.79 |
| drugtraff_yr_avg(96) | 5.25 | 4.79 |
| drugtraff_yr_avg(108) | 5.00 | 5.15 |

Non-Zero Weights for Synthetic Control

| Post | Weight |
|---|---|
| 113 | 0.16 |
| 115 | 0.13 |
| 132 | 0.04 |
| 133 | 0.01 |
| 312 | 0.10 |
| 331 | 0.00 |
| 332 | 0.11 |
| 424 | 0.30 |
| 525 | 0.03 |
| 713 | 0.02 |
| 724 | 0.12 |

Lower Park Heights – Nonfatal Shootings

Predictor Balance

| | Treated | Synthetic |
|---|---|---|
| homicide_yr_avg(12) | 0.17 | 0.21 |
| homicide_yr_avg(24) | 0.08 | 0.18 |
| homicide_yr_avg(36) | 0.17 | 0.26 |
| homicide_yr_avg(48) | 0.17 | 0.23 |
| homicide_yr_avg(60) | 0.17 | 0.32 |
| homicide_yr_avg(72) | 0.25 | 0.20 |
| homicide_yr_avg(84) | 0.17 | 0.20 |
| homicide_yr_avg(96) | 0.17 | 0.14 |
| homicide_yr_avg(108) | 0.08 | 0.18 |
| homicide_yr_avg(120) | 0.25 | 0.20 |
| shooting_yr_avg(12) | 0.42 | 0.57 |
| shooting_yr_avg(24) | 0.67 | 0.52 |
| shooting_yr_avg(36) | 0.08 | 0.45 |
| shooting_yr_avg(48) | 0.58 | 0.58 |
| shooting_yr_avg(60) | 0.83 | 0.82 |
| shooting_yr_avg(72) | 0.58 | 0.60 |
| shooting_yr_avg(84) | 0.42 | 0.36 |
| shooting_yr_avg(96) | 0.50 | 0.41 |
| shooting_yr_avg(108) | 0.58 | 0.43 |
| shooting_yr_avg(120) | 0.08 | 0.26 |
| wpnposs_yr_avg(12) | 1.08 | 0.77 |
| wpnposs_yr_avg(24) | 1.17 | 1.10 |
| wpnposs_yr_avg(36) | 0.67 | 0.84 |
| wpnposs_yr_avg(48) | 1.08 | 1.15 |
| wpnposs_yr_avg(60) | 1.50 | 1.14 |
| wpnposs_yr_avg(72) | 1.58 | 1.07 |
| wpnposs_yr_avg(84) | 0.67 | 0.89 |
| wpnposs_yr_avg(96) | 0.42 | 0.85 |
| wpnposs_yr_avg(108) | 0.83 | 0.81 |
| wpnposs_yr_avg(120) | 0.17 | 1.04 |
| drugposs_yr_avg(12) | 15.83 | 14.94 |
| drugposs_yr_avg(24) | 33.67 | 21.94 |
| drugposs_yr_avg(36) | 28.17 | 22.47 |
| drugposs_yr_avg(48) | 24.58 | 24.05 |
| drugposs_yr_avg(60) | 17.83 | 18.66 |
| drugposs_yr_avg(72) | 22.17 | 21.81 |
| drugposs_yr_avg(84) | 16.83 | 16.68 |
| drugposs_yr_avg(96) | 18.67 | 15.66 |
| drugposs_yr_avg(108) | 15.08 | 13.34 |
| drugposs_yr_avg(120) | 15.83 | 14.40 |
| drugtraff_yr_avg(12) | 11.08 | 10.95 |
| drugtraff_yr_avg(24) | 10.75 | 10.86 |
| drugtraff_yr_avg(36) | 7.17 | 6.25 |
| drugtraff_yr_avg(48) | 7.17 | 9.11 |
| drugtraff_yr_avg(60) | 4.17 | 6.60 |
| drugtraff_yr_avg(72) | 4.33 | 7.10 |
| drugtraff_yr_avg(84) | 4.17 | 6.14 |
| drugtraff_yr_avg(96) | 6.92 | 5.55 |
| drugtraff_yr_avg(108) | 6.17 | 5.34 |
| drugtraff_yr_avg(120) | 3.17 | 4.54 |

Non-Zero Weights for Synthetic Control

| Post | Weight |
|---|---|
| 124 | 0.08 |
| 412 | 0.01 |
| 611 | 0.01 |
| 613 | 0.11 |
| 614 | 0.23 |
| 715 | 0.11 |
| 732 | 0.08 |
| 814 | 0.25 |
| 815 | 0.02 |
| 834 | 0.01 |
| 933 | 0.09 |

Sandtown – Nonfatal Shootings

Predictor Balance

| | Treated | Synthetic |
|---|---|---|
| homicide_yr_avg(12) | 0.25 | 0.22 |
| homicide_yr_avg(24) | 0.08 | 0.18 |
| homicide_yr_avg(36) | 0.33 | 0.21 |
| homicide_yr_avg(48) | 0.25 | 0.21 |
| homicide_yr_avg(60) | 0.17 | 0.18 |
| homicide_yr_avg(72) | 0.17 | 0.19 |
| homicide_yr_avg(84) | 0.08 | 0.17 |
| homicide_yr_avg(96) | 0.08 | 0.19 |
| homicide_yr_avg(108) | 0.08 | 0.12 |
| homicide_yr_avg(120) | 0.17 | 0.17 |
| homicide_yr_avg(132) | 0.17 | 0.23 |
| homicide_yr_avg(144) | 0.00 | 0.15 |
| homicide_yr_avg(156) | 0.33 | 0.35 |
| shooting_yr_avg(12) | 0.42 | 0.46 |
| shooting_yr_avg(24) | 0.58 | 0.60 |
| shooting_yr_avg(36) | 0.75 | 0.52 |
| shooting_yr_avg(48) | 0.58 | 0.49 |
| shooting_yr_avg(60) | 0.17 | 0.42 |
| shooting_yr_avg(72) | 0.58 | 0.52 |
| shooting_yr_avg(84) | 0.42 | 0.35 |
| shooting_yr_avg(96) | 0.33 | 0.36 |
| shooting_yr_avg(108) | 0.33 | 0.30 |
| shooting_yr_avg(120) | 0.17 | 0.28 |
| shooting_yr_avg(132) | 0.08 | 0.30 |
| shooting_yr_avg(144) | 0.50 | 0.34 |
| shooting_yr_avg(156) | 0.75 | 0.66 |
| wpnposs_yr_avg(12) | 0.92 | 0.67 |
| wpnposs_yr_avg(24) | 1.33 | 0.77 |
| wpnposs_yr_avg(36) | 1.08 | 0.92 |
| wpnposs_yr_avg(48) | 0.50 | 0.76 |
| wpnposs_yr_avg(60) | 0.75 | 0.85 |
| wpnposs_yr_avg(72) | 0.58 | 0.86 |
| wpnposs_yr_avg(84) | 0.67 | 0.75 |
| wpnposs_yr_avg(96) | 0.50 | 0.67 |
| wpnposs_yr_avg(108) | 0.67 | 0.72 |
| wpnposs_yr_avg(120) | 0.33 | 0.74 |
| wpnposs_yr_avg(132) | 0.67 | 0.78 |
| wpnposs_yr_avg(144) | 0.42 | 0.75 |
| wpnposs_yr_avg(156) | 0.58 | 0.80 |
| drugposs_yr_avg(12) | 11.00 | 12.88 |
| drugposs_yr_avg(24) | 11.00 | 12.52 |
| drugposs_yr_avg(36) | 12.08 | 14.99 |
| drugposs_yr_avg(48) | 13.42 | 18.34 |
| drugposs_yr_avg(60) | 25.75 | 19.51 |
| drugposs_yr_avg(72) | 25.67 | 22.75 |
| drugposs_yr_avg(84) | 13.75 | 15.93 |
| drugposs_yr_avg(96) | 12.50 | 14.67 |
| drugposs_yr_avg(108) | 16.33 | 12.70 |
| drugposs_yr_avg(120) | 19.25 | 14.59 |
| drugposs_yr_avg(132) | 11.17 | 13.43 |
| drugposs_yr_avg(144) | 8.50 | 9.52 |
| drugposs_yr_avg(156) | 3.75 | 3.91 |
| drugtraff_yr_avg(12) | 7.42 | 7.86 |
| drugtraff_yr_avg(24) | 7.92 | 7.36 |
| drugtraff_yr_avg(36) | 4.75 | 5.50 |
| drugtraff_yr_avg(48) | 4.17 | 6.32 |
| drugtraff_yr_avg(60) | 7.75 | 6.36 |
| drugtraff_yr_avg(72) | 6.58 | 7.21 |
| drugtraff_yr_avg(84) | 5.92 | 6.42 |
| drugtraff_yr_avg(96) | 5.92 | 5.81 |
| drugtraff_yr_avg(108) | 7.50 | 6.18 |
| drugtraff_yr_avg(120) | 4.25 | 5.01 |
| drugtraff_yr_avg(132) | 3.08 | 4.74 |
| drugtraff_yr_avg(144) | 4.00 | 3.54 |
| drugtraff_yr_avg(156) | 3.58 | 2.38 |

Non-Zero Weights for Synthetic Control

| Post | Weight |
|---|---|
| 111 | 0.01 |
| 113 | 0.07 |
| 132 | 0.11 |
| 214 | 0.05 |
| 323 | 0.06 |
| 412 | 0.07 |
| 415 | 0.09 |
| 421 | 0.10 |
| 534 | 0.03 |
| 536 | 0.03 |
| 611 | 0.00 |
| 633 | 0.03 |
| 712 | 0.10 |
| 725 | 0.10 |
| 732 | 0.08 |
| 733 | 0.08 |

## Appendix C

Figure C1: Interview Guide for Ceasefire Interviews

Interview Guide for Ceasefire Interviews
February 11, 2018 (Version 2)

Thank you for meeting with me today. I am conducting research on the Ceasefire intervention in Baltimore between 2014 and 2016. I am interviewing a number of individuals who were involved with the planning and implementation of the intervention, and I would like to hear your thoughts about the intervention.

1. What are your initials?
2. What was your role as it related to the Ceasefire intervention?
3. Approximately how long were you involved with Ceasefire?
4. How many call-ins or custom notifications did you participate in?
5. What was your understanding of the role of law enforcement in targeting individuals identified by Ceasefire?
6. What criteria were used to determine when and which individuals were targeted for arrest?
7. What do you think were the successes of Ceasefire?
8. What do you think were the challenges of Ceasefire?
9. If the Ceasefire intervention were to return to Baltimore, what changes would need to be made in order for it to be successful?
10. Is there anything else you would like to share with me as it relates to Ceasefire?

Thank you very much for your time.

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## EDUCATION

***Doctor of Philosophy (PhD) in Health and Public Policy*** *Summer 2018*
Department of Health Policy and Management, Johns Hopkins Bloomberg School of Public Health, Baltimore, MD
Dissertation Research: "Examining Local Interventions to Reduce Gun Violence in Baltimore"
Relevant Coursework: Advanced Methods in Health Services Research (Analysis and Research Design); Observational Epidemiology; Statistics for Psychosocial Research; Multilevel Modeling; Analysis of Longitudinal Data; Causal Inference; Spatial Analysis and GIS; Politics of Health Policy; Communication Network Analysis; Concepts in Qualitative Research; Adolescents, Crime and Justice; Family Violence; Translating Research into Public Health Programs; Seminar on Health Disparities; Epidemiology of Substance Use and Related Problems

***Master of Public Health (MPH)*** *May 2013*
Johns Hopkins Bloomberg School of Public Health, Baltimore, MD
Capstone Project: "Handguns for Sale via the Internet: Easy Access for Illegal Buyers?"
Relevant Coursework: 4 terms of Biostatistics; 4-term Health Policy series (Social and Economic Determinants Health, Policy Formulation, Analysis and Synthesis, and Research and Evaluation Methods); Health, Poverty, and Public Policy in the U.S.; Public Health and the Law; Health Advocacy; Program Planning for Behavior Change; Introduction to the Risk Sciences and Public Policy

***Certificate in Injury Control*** *May 2013*
Johns Hopkins Bloomberg School of Public Health, Baltimore, MD
Relevant Coursework**:** Issues in Injury and Violence Prevention; Understanding and Preventing Violence; Design and Evaluation of Community Health and Safety Interventions; Graduate Seminars in Injury Research and Policy; Introduction to Persuasive Communications

***Bachelor of Arts (BA) in Psychology*** *June 2002*
Stanford University, Palo Alto, CA
Concentration: Mind, Culture, and Society
Dean's List honoree 11 of 12 quarters

## RESEARCH EXPERIENCE

***Urban Associate*** *The Urban Institute, Washington, DC* *March 2017 – present*
*Principal Investigator: Jocelyn Fontaine, PhD*
*Provide project management and research expertise on two multi-year projects with Urban's Justice Policy Center.*

- *West Baltimore Youth Violence Prevention Initiative, May 2017 – present*
  Facilitate problem assessment and intervention design and development with cross-sector partners and community members as part of the University of Maryland Baltimore's Bureau of Justice Assistance Byrne Criminal Justice Innovation award.
- *The National Initiative for Building Community Trust and Justice, July 2017 – October 2017*
  Managed field interview teams in Gary, Indiana, and Birmingham, Alabama, to conduct neighborhood surveys

on residents' perceptions of police and criminal justice systems in their cities as part of a six-city, multi-faceted intervention effort to improve community-police relations.

***Graduate Research Assistant*** *Johns Hopkins Center for Gun Policy and Research* *April 2013 – present*
Support numerous research projects led by the center director and faculty members.

- *Baltimore – Johns Hopkins Collaborative for Violence Reduction, January 2016 – present*
  Principal Investigator: Daniel Webster, ScD, MPH
  Provide technical assistance to the Baltimore Police Department and Mayor's Office to develop, implement, and evaluate myriad interventions aimed at decreasing violence in Baltimore.
- *Impact Evaluation of Maryland Firearm Safety Act of 2013, January 2016 – present*
  Principal Investigator: Cassandra Crifasi, PhD, MPH
  Assisted with qualitative survey design, implementation management, and data collection. Co-wrote and edited journal submission.
- *Gun Owner Attitudes Study, November 2013 – September 2016*
  Principal Investigator: Cassandra Crifasi, PhD, MPH
  Drafted Institutional Review Board proposal for study on gun owners' attitudes towards safe firearm storage and gun policies designed to protect unauthorized access to firearms. Assisted with coding of themes from focus groups with gun owners in Texas about their own storage and transfer practices.
- *Baltimore Homicide Review Commission, August 2014 – April 2016*
  Principal Investigator: Daniel Webster, ScD, MPH
  Facilitated the homicide review commission in Baltimore, which entailed establishing key partnerships and leading case reviews with Baltimore's Police Department, State's Attorney's Office, Health Department, and other law enforcement and criminal justice agencies, as well as community-based organizations and service partners, to identify patterns and trends that, if addressed, could prevent future homicides. Co-led focus groups, conducted quantitative analysis of the effectiveness of initiatives designed to reduce homicides and nonfatal shootings, and aided in the drafting of a report to city leaders on findings and recommendations.
- *Prison Inmates' Gun Acquisition Study, September 2013 – March 2014*
  Principal Investigator: Daniel Webster, ScD, MPH
  Performed statistical data analysis for an in-progress research project on prison inmates' gun acquisition methods.
- *State Firearm Licensing Laws, April 2013 – August 2013*
  Principal Investigators: Daniel Webster, ScD, MPH, and Jon Vernick, JD, MPH
  Researched state firearm licensing laws to understand differences in requirements in order to help inform Maryland state policymakers during their implementation of the Maryland Firearm Safety Act of 2013.

***Technical Advisor*** *CO Department of Public Health and the Environment, Denver, CO* *April 2016 – March 2017*
Lead Advisor, Johns Hopkins Bloomberg School of Public Health: Renee Johnson, PhD, MPH
Co-conducted a systematic review of the relationship between marijuana use and intimate partner violence among adults for the Retail Marijuana Public Health Advisory Committee with five team members.

## PROFESSIONAL EXPERIENCE

***Dissertation Editor*** *Johns Hopkins Bloomberg School of Public Health* *February 2017*

- Completed proofreading and copy editing of the dissertation, "Neighborhood Variation in the Rate of Child Welfare Contact," of Stacey W. Williams in the Department of Mental Health.

***Baltimore City Results Team Member*** *Mayor's Office, Baltimore, MD* *June 2015 – February 2016*

- Served on eight-member team responsible for reviewing all Baltimore Police Department and other city agency budget proposals related to public safety for fiscal year 2017 and providing recommendations to Mayor Stephanie Rawlings-Blake and the Baltimore City Department of Finance.
- Rated program enhancements and resource allocation requests on proposed value, effectiveness, and reach.

***Violent Crime Reduction Strategist*** *Mayor's Office, Baltimore, MD* *February 2013 – November 2015*

Under the direction of Baltimore Mayor Stephanie Rawlings-Blake and her then-Deputy Mayor of Emergency Management and Public Safety, coordinated initiative designed to dramatically and sustainably reduce violent crime through a comprehensive plan involving city agencies, nonprofit organizations, researchers, and community groups.

- Organized regular meetings with the Baltimore City Mayor, Police Commissioner, Health Commissioner, members of the Mayor's cabinet, heads of city agencies, and other key stakeholders to discuss crime trends and current/future strategies geared toward crime prevention and public safety enhancement.
- Collaborated with staff from the Mayor's Offices on Criminal Justice and Emergency Management/Public Safety to identify and help implement evidence-based, innovative solutions used in other local jurisdictions to address violent crime.
- Conducted research and policy analysis and evaluation related to youth violence, reintegration, intimate partner violence, gun crimes, and human trafficking to make recommendations to Baltimore policymakers for establishing and improving violence prevention programs.
- Assisted with grant proposal writing and implementation strategies to secure funding for juvenile justice and violence prevention initiatives aimed at reducing crime and recidivism rates.

***Senior Manager, Wellness Value Planning and Delivery*** *OptumHealth, Atlanta, GA* *February 2011 – June 2012*

Directed activities of client implementation, coach communication and training, coach process and technology enhancement, and consumer quality and value delivery evaluation for the 200-person Wellness Coaching Operations.

- Led team of project coordinators responsible for supporting Wellness Coaching Operations through new hire training, effective client updates, process and technology improvements, program execution, and evaluative tools.
- Developed and evaluated performance metrics to deliver training and enrichment programs to individual coaches, teams of coaches and supervisors/managers to drive consumer outcomes, client return on investment and productivity improvements from better processes and technology.
- Supported short- and long-term strategic business activities by identifying needs of the operation and translating operational data into actions and enhancements.
- Collaborated with business subject matter experts to understand existing business processes and future business needs to create key deliverables and process improvements.
- Researched and identified strategic growth and cost containment opportunities; communicated findings and provided recommendations to various levels of the organization, including directors, site executives, account managers, wellness consultants, and vice presidents.

***Operations Manager, Wellness Coaching*** *OptumHealth, Atlanta, GA* *August 2008 – February 2011*

Managed Wellness Coaching Operations in OptumHealth's Atlanta site, serving as liaison to clients and internal partner programs.

- Provided leadership, support, and quality training to Atlanta Wellness Coaching team, which serves hundreds of thousands of consumers through education, collaboration, and consumer activation in the areas of weight management, exercise, nutrition, diabetes, heart health, tobacco cessation, and stress management.
- Led implementation and expansion of Wellness presence in OptumHealth's Atlanta site from two to six designated and dedicated accounts and from six to twenty coaches.
- Led weekly team meetings, trainings, and monthly 1:1 feedback sessions with each coach, focusing on productivity and program outcome goals, employee engagement, interim and annual reviews, quality improvement, and career development.
- Facilitated successful URAC accreditation site review, resulting in the division's first full Comprehensive Wellness Program Accreditation.
- Improved coach performance and productivity through live call monitoring, real-time performance discussions, weekly metric reviews, case documentation evaluations, and call quality audits.
- Conducted hiring interviews and acted as primary leader on hiring offers and training plans for all new Wellness hires in Atlanta.
- Partnered with clinical managers, account executives, and product consultants to understand client-specific needs and develop or maintain processes that ensure truly integrative and co-managed care among the different health solutions teams.
- Communicated with upstream departments to address scheduling or utilization issues that could affect customer commitments and performance guarantees.

***Transportation/Logistics Manager, Sales Department*** *McMaster-Carr, Atlanta, GA July 2005 – June 2008*
Managed 25-member support team that provided day-to-day operational assistance to a 120-member Sales force at a multimillion-dollar industrial supply company.

- Implemented critical decisions regarding price breaks, supply chain management, expedited delivery, and other exceptional services for over 400,000 customers.
- Coordinated and monitored daily transportation logistics for over 10,000 parcel packages and 450 freight shipments to achieve next-day delivery to over 97% of customers in the southeastern region of the United States.
- Developed and maintained department staffing model that allowed 90% of work volume to be completed within one hour by aligning resources with fluctuating incoming volumes of work.
- Produced extensive training schedule for all new departmental staff and tracked developmental progress of all employees through weekly follow-ups, monthly mini-evaluations, and annual reviews.
- Reallocated responsibilities and streamlined processes to reduce process time by 30%.
- Presented weekly transactional topics regarding departmental development and improvements to directors and vice presidents.

***Operations Manager, Freight Operations*** *McMaster-Carr, Atlanta, GA January 2004 – July 2005*
Managed operations of the 20-member Freight department and its daily filling, packing and shipping of over 400 orders totaling approximately $400,000.

- Oversaw shipment planning and internal logistics management for all non-parcel orders.
- Gathered data and organized moving teams in preparation for the complete transfer of Freight Operations to a new facility.
- Maintained fastest and most accurate filling and shipping rates of all five Freight Operations departments nationwide while preserving low damage credit rates and satisfactory order presentation.

***Warehouse Logistics Supervisor, Parcel Packing*** *McMaster-Carr, Atlanta, GA March 2003 – January 2004*
Co-directed the assembly, packing, and shipping of over 5,000 packages daily; supervised eleven employees and one staff lead.

- Succeeded in breaking branch records for orders' overall time to ship and percentage of orders shipped under one hour.
- Reduced damage-related credits by 15% by identifying trends with highly damaged items and implementing operational procedures to address causative factors.
- Instituted new department staffing model based on the productivity of each employee, allowing for more accurate prediction of staffing requirements for projected order forecasts.

***Management Trainee, Marketing Department*** *McMaster-Carr, Atlanta, GA September 2002 – March 2003*
Performed market analyses to implement strategies for attracting high-potential customers and strengthening delivery network.

- Improved delivery to approximately 75,000 customers in 6 states by working with representatives from major package carriers to increase serviceability.
- Integrated shipping initiatives to begin same-day delivery to metropolitan Atlanta area and large cities within a 150-mile radius, resulting in faster delivery and 20% growth in those areas.
- Audited carrier invoices for billing discrepancies and discovered inaccuracies totaling $30,000 in erroneous charges; reported findings to the regional finance manager, controller, and assistant vice president.
- Reviewed and diagnosing all large dollar discrepancies during weekly inventory tasks.
- Worked closely with the Inventory Accuracy department to reduce inefficiencies in storage spaces and replenishment points.

## INSTRUCTIONAL EXPERIENCE

***Teaching Assistant*** *Johns Hopkins Bloomberg School of Public Health September 2014 – October 2017*
Facilitate instruction of graduate-level courses through management of administrative responsibilities, content development, lecture editing, guest lecturing, online course site development, scheduling of guest speakers, assignment creation and grading, and student support.

- *Graduate Seminar on Violence and Crime Prevention, 2nd Term, 2014-2015 and 1st Term, 2017-2018*
  Professor: Daniel Webster, ScD, MPH
- *Health Advocacy, 4th Term, 2014-2015, 4th Term, 2015-2016, and 4th Term, 2016-2017*
  Professor: Josh Horwitz, JD
- *Understanding and Preventing Violence, 3rd Term, 2014-2015, 3rd Term, 2015-2016, and 3rd Term, 2016-2017*
  Professor: Daniel Webster, ScD, MPH
- *Public Health and the Law, 3rd Term, 2014-2015, 3rd Term, 2015-2016, and 3rd Term, 2016-2017*
  Professor: Jon Vernick, JD, MPH
- *Master of Public Health Social and Behavioral Sciences Concentration, AY 2014-2015 and AY 2015-2016*
  Professors: Janice Bowie, PhD, MPH, and Caitlin Kennedy, PhD, MPH
- *Graduate Seminar on Occupational Injury Prevention, 3rd Term, 2014-2015 and 2nd Term, 2015-2016*
  Professors: Keshia Pollack, PhD, MPH & Cassandra Crifasi, PhD, MPH
- *Intro to Health Policy, 1st Term, 2014-2015 and 1st Term, 2015-2016*
  Professor: Sosena Kebede, MD, MPH
- *Issues in Injury and Violence Prevention, 1st Term, 2015-2016*

Professor: Jon Vernick, JD, MPH

- *Center for Injury Research and Policy Summer Institute, Summer Session, 2014-2015*
  Professor: Carolyn Fowler, PhD, MPH
- *Graduate Seminar on Injury Prevention and the Law, 4th Term, 2014-2015*
  Professor: Helaine Rutkow, PhD, JD, MPH, and Jon Vernick, JD, MPH

## GRANTS/AWARDS

*Dissertation Award* — *September 2017 – present*
Johns Hopkins Center for Gun Policy and Research

*Executive Alliance Emerging Women Leaders Scholarship* — *August 2017 – present*
Central Scholarship of Maryland

*Pre-Doctoral Traineeship in Drug Dependence Epidemiology Research (T32)* — *September 2015 – August 2017*
National Institute of Drug Abuse

*Pre-Doctoral Traineeship in Interdisciplinary Research in Violence (T32)* — *September 2013 – August 2015*
National Institute of Child Health and Development

*Health Resources and Services Administration Trainee Fellowship* — *April 2015 – August 2015*
Department of Health and Human Services

*National Violent Death Reporting System Surveillance Academy Scholarship* — *June 2015*
Safe States Alliance

## PUBLICATIONS

Webster, DW, **Buggs, SAL**, Crifasi, CK. (2018). Estimating the Effects of Law Enforcement and Public Health Interventions Intended to Reduce Gun Violence in Baltimore. Johns Hopkins Center for Gun Policy and Research. https://www.jhsph.edu/research/centers-and-institutes/johns-hopkins-center-for-gun-policy-and-research/publications/JHSPH-Gun-Violence-in-Baltimore.pdf.

Zeoli, AM, McCourt A, **Buggs S**, Frattaroli S, Lilley D, Webster D. (2017). Analysis of the Strength of Legal Firearms Restrictions for Perpetrators of Domestic Violence and Their Association with Intimate Partner Homicide. *American Journal of Epidemiology*, kwx362, https://doi-org.ezp.welch.jhmi.edu/10.1093/aje/kwx362.

Crifasi CK, **Buggs SAL**, Chosky S, Webster DW**.** (2017). The Initial Impact of Maryland's Firearm Safety Act of 2013 on the Supply of Crime Handguns in Baltimore. *The Russell Sage Foundation Journal of the Social Sciences*, 3(5): 128-140.

Webster DW, **Buggs, SAL**. (2017). Can an Efficacious Strategy for Curtailing Illegal Drug Sales Be Counted on to Reduce Violent Crime? *Criminology and Public Policy*, 16(3): 821-825.

Milam AJ, **Buggs SA**, Furr-Holden CDM, Leaf PJ, Bradshaw CP, Webster D. (2016). Changes in Attitudes Towards Guns and Shootings Following Implementation of the Safe Streets Intervention. *Journal of Urban Health*, 93(4): 609-626.

Frattaroli S, **Buggs SAL**. (2015). Decreasing Gun Violence: Evidence-Based Social and Public Health Interventions. In LH Gold and RI Simon (Eds.), *Gun Violence and Mental Illness*. Arlington, VA: American Psychiatric Association Publishing.

Webster D, Meyers JS, **Buggs, SA**. (2014). Youth Acquisition and Carrying of Firearms in the United States: Patterns, Consequences, and Strategies for Prevention. *Proceedings of Means of Violence Workshop, Forum of Global Violence Prevention, Institutes of Medicine of the National Academies.*

## PRESENTATIONS

**Buggs, S.** (2017). "The Role of the Community in Preventing Violence in Baltimore." Keynote speaker at Baltimore Mayor Catherine Pugh's Call to Action Baltimore Grassroots Leaders Retreat.

**Buggs, S.** (2016). "Effects of Drug Law Enforcement on Gun Violence in Baltimore." Poster presenter at the American Public Health Association Conference (APHA), Denver, CO.

**Buggs, S.** (2016). "Understanding and Addressing Violence Through a Public Health Lens." Panel presenter at the Healing Justice Alliance Conference, Baltimore, MD.

## LEADERSHIP AND SERVICE

| | | |
|---|---|---|
| *Student Board Representative, Society for Advancement of Violence and Injury Research* | | *April 2018-present* |
| *Dean Search Committee* | *Johns Hopkins Bloomberg School of Public Health* | *2016-2017* |
| *Executive Committee/Task Force Member, My Brother's Keeper – Baltimore* | *City of Baltimore* | *2015-present* |
| *Career Fair Representative* | *Johns Hopkins Bloomberg School of Public Health* | *2015-present* |
| *Mentor, Diversity Summer Internship Program* | *Johns Hopkins Bloomberg School of Public Health* | *2013-present* |
| *Alumni Interviewer and College Fair Representative* | *Stanford Alumni Association* | *2003-present* |
| *Board Member, Student Coordinating Committee* | *Johns Hopkins Bloomberg School of Public Health* | *2014-2015* |
| *Vice President, Black Graduate Student Association* | *Johns Hopkins Bloomberg School of Public Health* | *2012-2013* |
| *Communications Chair/Board Member* | *Stanford National Black Alumni Association* | *2009-2012* |
| *Co-President, Stanford Black Alumni Association – Atlanta* | *Stanford Alumni Association* | *2007-2012* |

## PROFESSIONAL DEVELOPMENT

***UnitedHealth Group***

- Foundational Quality
- Cultural Competence
- Consumer Data Control
- Situational Leadership

- People Styles at Work
- Building Winning Teams

***American Management Association***

- Handling Difficult Conversations

***Dale Carnegie***

- Successfully Leading People

## PROFESSIONAL AFFILIATIONS

- American Public Health Association
- Society for Advancement of Violence and Injury Research
- American Society of Criminology

## SOFTWARE PROFICIENCIES

- Microsoft Office Suite
- STATA Data Analysis and Statistical Software
- ArcGIS Mapping and Statistical Analysis Software
- UCINET Social Network Analysis Software
- Netdraw Social Network Visualization Software